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Science to grow again?

WHAT is the minimum rate at which expenditure by research councils should grow in real terms in the next few years to sustain the vigour of the scientific community in Britain? A difficult question, and in some ways a meaningless one. One single aggregated figure for growth would have to cover a wide range of activities from research of great timeliness through to research that might just as well be terminated right now; it would also have to make allowance for discoveries yet to be made but which will call for the commitment of new resources in years ahead. The Advisory Board for the Research Councils (ABRC), however, make a guess at a figure in their just-issued report for the years 1976–78 (HMSO; 70p): 4%. The report gives a valuable survey of past fluctuations in financial support, from the remarkable days of 13% real growth in 1966, through the period of at-best no growth, at-worst cuts of a few per cent in the mid-1970s to the slightly calmer waters of about 1% annual growth promised for the next three years.

What lessons have been learnt from this period? That salaries drift upwards in real terms by up to one per cent each year simply because of increased seniority; that the cost of equipment drifts up also, especially now that much which was bought during the growth period of the 1960s is obsolescent—excepting that computers are still getting cheaper; that without a little financial elbow-room all money gets locked into continuing projects, buildings and staff, and new initiatives never get the start they need; that if too much growth is pumped into the system too quickly, the money injected either cannot be spent and is returned to the Treasury, or is spent hastily and not necessarily wisely. From all these considerations, the figure of 4% looks realistic. Whether it will sell in Cabinet in inflationary times, or with a possible change of government is another matter.

But whilst research done through research councils has been subject to severe restraints these past few years, and whilst the other arm of the 'dual-support system'—financial assistance for universities through the University Grants Committee (UGC)—has, if anything, experienced even greater difficulties in making ends meet, defence research and development has shown relative resilience. The ABRC reports valuably bring together the nation's R & D expenditure in one table. In

1969/70 total government R & D cost £604 million; it now (1978/9) costs £1,735 million. The proportion of this sum which has been spent on defence R & D has risen from 40% in 1969/70 to 50.5% in the current financial year.

It might be argued that during a period in which the government pared down on major civil commitments such as its contribution to the European Launcher Development Organisation, and research on Concorde, it would be natural for defence R & D to seem to grow in significance. One way to test this would be to see whether money for research councils and for the research side of University Grants Committee (UGC) support (which can only be an estimate) has kept pace with defence R & D funding. The calculation is somewhat complicated by the redistribution of funds during the Rothschild re-organisation, so we do it for the Science Research Council only, this council not having had to transfer any of its funds over the past several years to customer departments. The ratio of defence R & D expenditure to SRC plus UGC research funds was 2.55 in 1969/70, dipped as low as 2.39 in 1974/5, but now stands at 2.93. The conclusion is inevitably that defence R & D has not in recent years experienced quite the privations that much of civil science has had to undergo.

Is it right that half of all government expenditure on R & D should go to defence? No-one (as far as we know) sits in Whitehall and preordains percentages that would be appropriate for research in different areas—relative movements are therefore much more likely to reflect changing conditions within individual ministries than any clear-cut policy decision. Nevertheless the remorseless upward trend in importance of the defence R & D figure should not go unremarked. It is next to impossible to tell whether the £875 million spent this year gives value for money, but anecdotal evidence—what little there is—suggests that areas of real excellence in defence R & D establishments are few and far between, and that the general level of competence and drive is not high. Mechanisms for weeding out inferior quality in civil science by peer review are by no means perfect, but at least they exist. Do we know that within a closed organisation, whose employees possess excellent job security, adequate steps are taken to protect the taxpayer's interest? □



Environmentalists fight use of 'untested' pesticide

The EPA has been accused of playing politics over Mississippi's fire ant 'emergency'. Clive Cookson reports

ENVIRONMENTALISTS are fighting tenaciously to prevent the use in the southern United States of an untested and allegedly carcinogenic pesticide. They say the Environmental Protection Agency approved the pesticide for political reasons and against the advice of its own scientists.

Twice last year the EPA agreed to requests by the state of Mississippi to use Ferriamicide, a new organochlorine insecticide, as an emergency weapon against fire ants. Each time the Environmental Defense Fund persuaded a federal court to delay the EPA approval. Last month the EPA announced a third time that it would permit Mississippi to apply Ferriamicide to a limited area this spring. But within a fortnight the EDF persuaded the agency to delay the approval again—this time without court action—by drawing its attention to some Canadian research results.

Ferriamicide, developed by Mississippi in a state-owned chemical plant, is a combination of Mirex*, imported from Brazil, with an amine and ferrous chloride to speed up its photodecomposition in the environment. But, according to a recent report by Dr David Villeneuve, head of the Environmental contaminants section of Health and Welfare Canada (HWC), one of Ferriamicide's main decomposition products, is photo-Mirex, which is even more toxic than Mirex, the active ingredient of Ferriamicide. Pure Mirex was the chief insecticide used against fire ants until the EPA banned it in 1976 after studies indicated that it causes cancer, birth defects, and other toxic effects in mammals.

EDF general counsel William Butler says the Canadian study was widely known before the EPA's most recent approval of Ferriamicide—Dr Villeneuve presented his work at several scientific meetings—but the EPA was apparently not aware of it.

According to the EDF, the agency's whole attitude to Ferriamicide has been influenced by the desire of senior EPA administrators to appease the powerful southern politicians who have been pressing very hard for its approval. Fire ants were accidentally introduced from South America in the 1920s and have spread remorselessly across the South, biting humans and animals and impeding agriculture with their mounds. Controlling the ants has become a big political issue in the nine southern states now infested.

Almost every southern senator and

congressman, as well as state and local politicians, joined the campaign to win EPA approval for Ferriamicide. A public relations drive led by Jim Buck Ross, Mississippi's Agriculture Commissioner, persuaded 20,000 people from the state to write to the agency in support of Ferriamicide.

EPA files, which the Environmental Defense Fund obtained during last year's litigation, show that the agency was highly sensitive to this political pressure, though EPA officials insist that the decisions to approve Ferriamicide were not influenced by political considerations. Barbara Blum, EPA deputy administrator wrote on one internal note. "If it's a political decision we want to make the most of it." She was apparently referring to the opportunity to win more favourable treatment for the EPA and for the Administration's pesticides legislation in Congress—the Senate and House committees which oversee the agency and fix its budget have many prominent members from the South.

None of the scientific branches of the EPA pesticides registration division supported their senior administrators' decisions on Ferriamicide. Some, including the chemistry and fish and wildlife branches, actively opposed them. Nevertheless, Dr Blum ruled that the fire ant "emergency" justified Mississippi's use of its unlicensed and virtually untested pesticide, though she did impose more stringent safeguards than the state proposed. (The eight other infested states lodged similar requests to apply Ferriamicide over a total of 17 million acres, but the EPA has not yet ruled on their cases.)

Dr Blum accepted Mississippi's argument that there is no effective alternative treatment for large, heavily infested areas, although one or two US chemical firms are reported to be developing new ant-killers which may be available soon. The environment-

alists argue, with the support of some agricultural scientists, that Mirex itself has not halted the spread of fire ants; the area infested has increased from 30 million acres in 1962, when Mirex was first used, to 190 million acres in the late 1970s.

According to William Butler of the EDF, the only result of Mississippi's highly visible war on the ants—involving helicopters and converted B17 aircraft—has been political. Voters can see and hear that the state and federal governments are doing something about the insects which are making their lives miserable. Mr Butler likes to compare the psychology behind the war—and its practical results—with the US attempts to bomb the Vietnamese into submission.

The uncomfortable position of the EPA—under fire from the environmentalists on one side and from the politicians and farmers on the other—became more embarrassing when the head of the US Government's other major regulatory agency for toxic chemicals, the Occupational Safety and Health Administration, criticised the latest EPA authorisation of Ferriamicide in Mississippi. What Eula Bingham, Assistant Secretary of Labour responsible for safety and health, objected to was a condition imposed by the EPA that no women of child-bearing age should handle Ferriamicide. OSHA officials are said to fear that this sex bias may be used or quoted as a precedent by chemical firms wishing to exclude women from their work forces.

They also doubt whether the ban on women handling the pesticide makes any sense. Research has indicated that Mirex damages the testes of male rats and may therefore cause fathers to pass on birth defects to their children. Logically, then, only the elderly and sterile should be allowed to use it.

*Chemically Mirex is dodecachlorooctahydro - 1,3,4-methano-2H - cyclobuta-(CD) pentalene. Photo-Mirex is 8-monohydro-Mirex.



UK union attacks GMAG bureaucracy

THE Association of University Teachers (AUT) in the UK is worried about the paperwork its members could face if the Genetic Manipulation Advisory Group's (GMAG) plans to include self-cloning experiments within its remit (see below) are adopted. Three AUT representatives expressed their concern to members of parliament last week when they gave evidence to the House of Commons Select Committee on Science and Technology which is currently examining genetic engineering.

Dr David Sherratt of Sussex University, one of the AUT representatives, explained to *Nature* after the meeting that most of the concern centres on the interpretation of part of the document published in *Nature* last year (9 November, page 104) on proposed new guidelines for the UK. The document states that laboratories intending to do self-cloning experiments should provide "a block notification" of the experiments to be done over a year, an undertaking to work in category 1* and a retrospective "detailed log of all experiments carried out".

The latter point causes Dr Sherratt most concern. "In our laboratory", he says "12-15 of us are using these techniques (cloning *E. coli* genes into *E. coli* organisms) every day." Providing GMAG with a detailed log of all such experiments (up to 1,000 in a year) could make the amount of bureaucracy the laboratory has to deal with "absolutely enormous", he claims. As yet, however, no-one knows whether a single sentence on each experiment will satisfy GMAG or whether it will require short papers. "It all depends on what they want", says Dr Sherratt.

Under the Williams guidelines, which still operate, his laboratory, together with most others, does not at the moment notify GMAG of self-cloning

experiments. Scientists have generally interpreted GMAG's definition of genetic manipulation as excluding self-cloning. During 1978, however, the Medical Research Council (MRC) indicated that self-cloning experiments should be considered by GMAG. It suddenly started to make the award of some grants for work involving self-cloning experiments conditional on those experiments being notified to GMAG. GMAG went along with this. Prior to 1978, it would seem that the MRC and GMAG had agreed with scientists that these experiments did not count as genetic manipulation.

The trouble is that GMAG's definition of genetic manipulation is ambiguous with regard to self-cloning, so it could be argued that both the MRC's interpretation and that of the scientists are correct. Even GMAG admits that it is ambiguous. Its remit is defined as: "the formation of new combinations of heritable material by the insertion of nucleic acid molecules produced, by whatever means, outside the cell, into any virus, bacterial plasmid or other vector system so as to allow their incorporation into a host organism in which they do not naturally occur but in which they are capable of continued propagation". The chief difficulty, says Dr Sherratt, arises in the meaning assigned to "they" in the phrase "in which they do not naturally occur". If it refers to the "new combinations", then all self-cloning experiments are included in the guidelines. If, on the other hand, it refers to the "nucleic acid molecules", as most scientists have assumed it does, then all self-cloning experiments are exempt from regulation. This is the point that GMAG has recently been debating.

Judy Redfearn

GMAG wants self-cloning notification

IN a controversial decision last week, Britain's Genetic Manipulation Advisory Group (GMAG) has opted to include self-cloning in its definition of "genetic manipulation", but to slacken its notification procedures for such experiments. However GMAG has not yet issued a formal statement of its new views, and will not do so until after 16 March, when it meets to decide a final form of words.

That meeting will require decisions on three crucial issues: first, what containment conditions will be required for self-cloning (and related experiments); second, what experiments to include in this slackened category; and third, what notification procedures to demand.

At present, GMAG feels that "good microbiological practice" will be sufficient, somewhat more relaxed than the present Category 1; that three or four systems could be included, such as *E. coli* in *E. coli*, and experiments likely to be undertaken in teaching; and that the notification should be more of a formality than a chore. But precisely where the group stands on these matters is not decided.

For comparison the revised guidelines of the US National Institutes of Health clearly exempt from any control all self-cloning experiments, experiments involving donor-host pairs that naturally exchange DNA, and some cloning experiments with viral DNA in viral vectors.

Robert Walgate



Britain snowed up, January 1979

World climate conference turns to the weather

IT is the short term variability of climate—to most people, year-to-year variation in 'the weather'—that matters most, not the longer term 'climatic' variation with which some (perhaps especially some of the environmentalists) are more deeply concerned. This is a first conclusion to be drawn from the "Conference of Experts on Climate and Mankind" convened recently in Geneva by the World Meteorological Organisation.

Certainly, the effects of short-term variability are serious enough. It was one such phenomenon, the Sahel drought of 1968-73, which in fact brought the need for some sort of climate conference to the fore.

Figures given by Robert Kates of Clark University indicated that between them floods, tropical cyclones and drought cost the world a good \$30 billion each year, claiming the lives of some 250,000 people, 95% of them citizens of the Third World. While the absolute costs of these disasters are many times greater in developed than in underdeveloped countries, in terms of GNP the situation is reversed: "Climatic hazard impacts poor countries 20-30 times the rate of rich countries," Kates suggests.

While the need for research on short-term variability was recognised as the immediate research priority, there was inevitably a great deal of discussion at the meeting of the possibility (rather than the probability) of medium term changes. This brought up the debate between the "ice age" prophets, and those who see the danger of a global warming as the more immediate threat.

By the end of the meeting, both could perhaps agree that their points had been met; there seems to be a general consensus that the Earth is at the start of a potential cooling period of perhaps 10,000 to 20,000 years, possibly not an ice age in the sense of those recorded in geological time, but still a major, long-term change.

At the same time, there was no doubt of the feeling of the meeting as a whole, regarding the possible combined effects of CO₂ emissions and those of other pollutants such as nitrous oxide and many chlorine compounds, including the already notorious chlorofluoromethanes. How little is known about the real effects of all these substances is evident from a note in the report of the working group concerned, that "it is possible that some processes could lead to a cooling" (rather than a warming as with CO₂) "of the atmosphere".

Despite this uncertainty, Ralph d'Arge of the University of Wyoming offered estimates of the economic costs of climatic change. Giving sets of figures for a drop of 1 °C, he calculated that losses in rice production could only be offset by an investment of \$19 billion—and rice is the staple crop of the majority of the world's poorest people.

Yet perhaps even more significant, as indicating the knife-edge of climate on which many national economies are balanced, are his figures for softwood

production in USSR: a drop of 1 °C would require an offsetting investment of \$28 billion, whereas only a half degree rise would give a \$13 billion bonus.

Where urban wages are concerned, this differential is even more marked: for the United States alone, d'Arge's figures jump from a loss of \$73 billion to a rise of \$31 billion. So it is not surprising that another conclusion of the conference was that planners, in the industrialised countries as well as those of the Third World, should take climatologists far more into their confidence than they do at present.

Not irrelevant in this connection was a response made to B. J. Mason, Director of Britain's Meteorological Office, when he remarked that variability such as the hot dry summer of 1976 need not be so disastrous: whereas certain British crops did badly, others so improved that the total loss was slight. It was the sole Iranian at the meeting who pointed out that perhaps this was because many crops grown in Britain are at the limit of their natural range, and need a warm summer to be really profitable.

Fascinating data on medium term climatic variation was put forward in the Chinese paper, based on studies of fluctuations over much of the last 5,000 years. Mean temperatures are believed to have varied over a range of two to three degrees—as great as that foreseen under a prolonged "greenhouse

effect" today. At the same time, Chinese scientists studying more recent records believe they have identified a number of short-term cycles of from two or three to 11, 22, 36 and 80 years for various parameters in different parts of their vast country with, it is admitted, an extraordinarily wide range of normal variation.

Inevitably, a good deal of attention was paid to the use of computers as the main tools in forecasting, especially perhaps over longer ranges of time, although it was agreed that there is an urgent need for better models to work to. Moreover, although no one was so crude as to point this out, an industrialised country such as Britain may well spend as much on a single giant computer as many, if not most, Third World countries have for their entire annual budget in meteorological services.

This point, at least, was reflected in the final Declaration of the Conference, with a call for assistance to these countries through training and the transfer of appropriate technologies without which they cannot participate fully in the proposed World Climate Programme. This programme, a bare outline of which was appended to the Conference Declaration, will be discussed by the WMO Congress when it meets from 30 April for four weeks, with a view to reaching decisions about content and funding.

Peter Collins

Call for world (non-nuclear) energy organisation

THE potential dangers of the rapid growth of nuclear energy on an international scale are so great that there is an urgent need to establish a global body to co-ordinate and stimulate the development of non-nuclear sources of energy, according to Dr Joseph Rotblat, emeritus professor of physics at the University of London, and a leading member of the Pugwash organisation.

Speaking at a meeting in London last week organised by the environment division of the Institute of Biology, Professor Rotblat said that although a global energy body already existed in the International Atomic Energy Authority, it existed to promote one form of energy—nuclear. No similar body existed to encourage other energy forms. "It is high time that the balance was restored", he said.

A large expansion of nuclear power would greatly increase the chance of nuclear war, said Professor Rotblat, since almost every nation would have access to plutonium, and would also have the technology necessary to make nuclear weapons. "In the long run nuclear energy is not compatible with the survival of civilisation", he said.



Fortunately the recent slowdown in global demands for energy meant that the expansion of nuclear energy was likely to be much slower than earlier predicted. However the risks of proliferation were still increasing, particularly since several countries had made the export of nuclear energy an important part of their economy, while the demand in third world countries had been stimulated by the "promotional activities" of the International Atomic Energy Agency.

"The most positive thing we could do to reduce the risk of nuclear war would be to encourage countries to opt for non-nuclear sources of energy. A global organisation devoted to this end would be important to give the same aura of respectability to non-nuclear energy sources as is at present given to nuclear energy", Professor Rotblat said.

Professor F. W. Spiers of the University of Leeds told the meeting that measuring background levels of radiation provided an important yardstick for studying the health effects of society's nuclear activities. Whether background radiation was a source of cancer was an open question, he said. "Although we would not anticipate it to have much effect when there are so many other causes of disease".

Levels of background radiation also provided a lower limit for the 'doubling dose' for different cancers, since if this dose was estimated at less than the background level, it would imply that the background radiation was responsible for more than the observed incidence of cancer.

David Dickson

Soviet space effort is strictly for the cash, not the glory

ACCORDING to TASS, the flight programme of Soyuz-32—launched on 25 February—includes docking with the Salyut-6 orbiting station and checking it for a possible further mission aboard the station. This suggests that the Soviet planners are anxious to get their last kopek's-worth out of Salyut-6, which has already been in orbit for almost 18 months.

Although the finances of the Soviet space programme are never published, current Party doctrine stresses that all branches of science should direct their research towards the practical needs of the national economy. The economic pay-off from the Soviet space programme was therefore the keynote of a press conference in Moscow last month given by chief Training Officer Vladimir A. Shatalov and cosmonaut Vladimir Kovalenkov.

According to Shatalov, "the greatest economic impact, the greatest benefit so far can be derived from research in near-Earth space and in the operation of orbital stations". This, he said, would remain the basis of the Soviet space programme for the next few years, because such questions "really do bring considerable, obvious benefits even now".

Illustrating such benefits, Kovalenkov mentioned observations from orbit of dust storms and meteorological and oceanological processes. When observations of the oceans began, he and his fellow-cosmonauts were not told what they were supposed to be observing. They had to work out by trial and error such practical matters as the best porthole to use in relation to the Sun's position. After some time, he said, it was possible to understand "to some extent" the ocean currents, and hence to identify resources of phyto- and zooplankton—to the ultimate benefit of the Soviet fishing fleet.

Observations from orbit of ocean vortices, Kovalenkov added, had led to an oceanological expedition to the Atlantic last summer. From subsequent reports, it appears that this expedition established the existence of isolated closed spiral currents, with diameter up to 200 miles. However these are predominantly in the Bermuda Triangle area, and the unbiased observer should remember that the Soviets, being good dialectical materialists, are at pains to find a physical explanation of the supposed Bermuda Triangle disappearances.

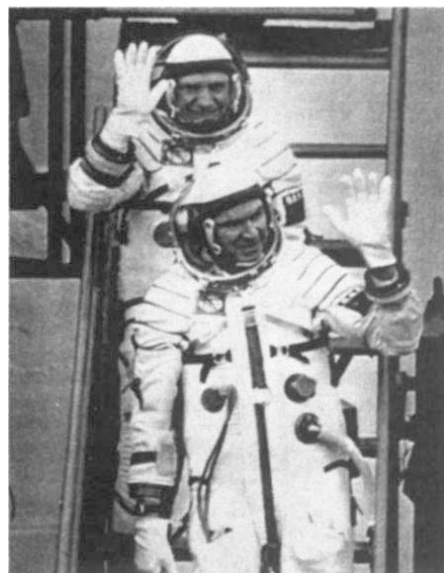
The observation of such processes from space will be a major task of the first Soviet unmanned oceanographic

satellite, Kosmos-1076, launched on 12 February. Soviet space planners have always stressed the great potential of unmanned craft and probes. The US Apollo moonflights, for example, were presented in the Soviet media as a waste of funds. The *Luna* spacecrafts and *lunokhod* rovers, it was stressed, could do all that a manned Soviet mission might do, at half the cost. Kovalenkov's remarks would suggest, however, that without the recent long-term manned Salyut missions, the programme of the unmanned Kosmos-1074 might well have been a somewhat hit-and-miss affair.

Earthquakes are another Soviet concern. A team at the State Nature Centre are using photographs from the Salyut missions to predict earthquakes in the Soviet seismic zones. A spokesman from the centre explained recently on Moscow radio that the seismic zones were first observed visually by Yuri Gagarin. Since some 15% of the terrain of the USSR is seismically active, while some 30% of the population live in these areas, earthquake prediction from satellite observations, he said, could produce "tremendous" savings in construction costs. Preliminary work along these lines was begun by the Salyut-6 crews last year.

Even the exotic triple Cd-Hg-Te semiconductor crystals produced about Salyut-6 in the joint Soviet-Polish "Syrena" experiment have their practical use. They have been handed over to the Polish Institute of Mining Safety, to be used in producing infrared-sensitive pyrometers.

Salyut, it would therefore seem, is paying its way. Indeed, Shatalov himself admitted as much in a recent interview with the Bulgarian daily *Otechestven Front*. Much of his state-



Soyuz-32 send off: Cosmonauts Lyakhov (top) and Ryumin before Sunday's launch.

ment was, naturally, devoted to the training of the Bulgarian cosmonaut candidates, in the Interkosmos programme, but in referring to the work of the Salyut crews "including the international ones" carrying out extremely valuable observations for the national economy, he implied that, in his opinion, from now on the programme would be operating essentially on a break-even basis.

Looking to the more distant future, Mr Shatalov noted that, with bigger spacecraft, there would be a stricter division of crews into scientific and operational personnel. This would cut considerably the training demands on the individual members of the crew who would now need to qualify only as scientists or, alternatively, as spaceship operatives. Ultimately, he said, a whole "Aeroflot-type" back-up organisation would have to be set up as well, to handle the terrestrial processing of the flights.

Vera Rich

Space shuttle may be six months late

A STUDY group of the US National Academy of Sciences predicts that engine problems mean the space shuttle's first flight, officially planned by NASA for November this year, is now unlikely to take place until April 1980.

In a report prepared at the request of the Senate committee on science, space and technology, the group says it is particularly concerned that the engine that will actually fly in the space shuttle will not be the same configuration as that currently being tested, and that, in the circumstances,

plans to perform flight certification tests on the engine, which will be used among other things to help launch the European Space Agency's Spacelab, are premature.

"The only rational option, for safety's sake, is to reschedule the formal flight certification until the configuration is fixed," Professor Eugene E. Covert, professor of aeronautics and astronautics at the Massachusetts Institute of Technology and chairman of the National Research Council group which prepared the report, said last week. □

news in brief

Supreme Court to hear benzene exposure case: The US Supreme Court has agreed to review an appeal court ruling which struck down the Occupational Safety and Health Administration's strict new limit for benzene exposure in the workplace. The Fifth Circuit Court of Appeals in New Orleans—whose reputation is pro-industry and anti-environmentalists—decided that the limit was invalid because OSHA did not provide a reasonable cost-benefit analysis for it. The industrial groups which challenged the limit estimated that it would cost them \$500 million to comply with the new standard and claimed there was no evidence that lives would be saved by it. The Government argued unsuccessfully that current scientific knowledge was insufficient for OSHA to predict accurately the number of lives saved by a given reduction in exposure to harmful chemicals. If the Supreme Court upholds the decision, the basis for government regulatory activities could be seriously undermined.

Rothschild's Dimbleby lecture on risk is attacked: "In view of the highly biased nature of Lord Rothschild's lecture we were shocked to learn that the Atomic Energy Authority is distributing copies of (the lecture) through its liaison office in Washington and we call upon you to stop its distribution forthwith" said the UK office of Friends of the Earth in a letter to UKAEA chairman Sir John Hill last week. Czech Conroy, Energy Campaigner of FOE, first raised the protest on 23 February at a lecture given by the UKAEA's health and safety adviser, Professor F. R. Farmer, to students at the Royal College of Art. Conroy pointed out that Rothschild's talk was based in part on "the severely discredited" Inhaber report prepared for the Atomic Energy Board of Canada on the risks of energy production and that Rothschild's talk had been widely criticised in the scientific press. Conroy also protested that in spite of these criticisms the Authority had published the lecture without comment in its monthly magazine, *ATOM*. The editor of *ATOM*, who happened to be in the audience, offered to publish a critique of the talk and Friends of the Earth requests in its letter that this be agreed formally. The UKAEA is not yet prepared to comment on the request.



Lord Rothschild

Social Audit audits UK food and drug companies: Social Audit Ltd, a London-based non-profit organisation, has issued a stinging critique of British drug company marketing practices in the Third World. Supporting the World Health Organisation's criticism of the selling of "inessential" drugs in Third World markets (8 February), author Charles Medawar further cites the sale of drugs specifically banned in Britain and suppression of information about dosage and side effects as examples of "disgraceful standards of advertising and marketing by UK companies in the developing countries". Medawar emphasises that drug expenditure, typically 20–50% of a country's health budget, can do little to ameliorate health problems caused by poor sanitation and malnutrition. Preventative measures and ultimately "political and economic solutions" are needed, he says. The Association of the British Pharmaceutical Industry calls the report "an unjustified smear". The ABPI says that, given the confused state of research, there are justifiable differences in the requirements set by different governments.

According to the Politics of Health Group of the British Society for Social Responsibility in Science, drug companies are the most profitable industrial enterprises. In the UK for 1975, rates and returns were estimated at 21.4% (Industrial chemicals were second at 16.2%). In 1972, Boots and Beechams showed 54% and 41% returns respectively on capital employed.

(*Insult or injury?* by Charles Medawar. *Social Audit*, 9 Poland St, London W1V 3DG; £1.50.)

German President draws attention to jobs problem: The President of West Germany, Walter Scheel, addressed the Wissenschaftsrat ("Science Council") that advises government on higher education policy on 25 January about the employment problems of young scientists. He hinted that the Heisenberg Programme was not a complete solution. This programme was recently devised by the Deutsches Forschungsgemeinschaft to keep at university those who had won their "habilitation" (right in principle to a university post) but had no job. Only some 70 people had been awarded Heisenberg fellowships so far. "Industry as well ought to be at least aware of this problem", Scheel said. "Perhaps there will be an opportunity to increase exchanges between industry and the universities." However, he added: "We cannot easily refute the claim that not enough basic research is being done in the Federal Republic." And casting a glance at the well-established principle of academic freedom in German universities, he said: "It is the right and duty of the state to insist upon some coordination in basic research". However this would not reduce the freedom and dignity of research. (*Wirtschaft, Wissenschaft, Politik* 5 February 1979.)

Porton Down transfer causes redundancies: The transfer of the Microbiological Research Establishment from the control of the Ministry of Defence to the Public Health Service Laboratory Service Board is expected to reduce the staff by 25% from 300 to 223. Although called "natural wastage", the redundancies include at least 10 senior principal scientific officers within five years of retirement. Most of the job losses are being contested by the Transport and General Workers Union which represents the laboratory assistants involved in the transfer. The Ministry of Defence says that no jobs will be lost through the move except "for some who want voluntary redundancy".

Oil from coal feasibility study launched in UK: The National Coal Board and the Department of Energy of the UK have signed a financial agreement to cover the cost of creating an engineering design and feasibility study for the construction of two coal extraction demonstration plants capable of processing 25 tonnes of coal per day. The two processes which have been developed to the laboratory stage are liquid solvent extraction (petrol, diesel fuel, kerosene) and super critical gas solvent extraction, a new process that will produce the aromatic compounds, benzene, toluene and xylene, as well as chemical feedstocks for plastics, rubbers, artificial fibres and paints. The Government will put £800,000 towards the total cost of £1.2 million. The actual cost of construction and operation of the two plants, to begin in 18 months, is estimated to be £30 million. Sir Derek Ezra, chairman of the National Coal Board said at the signing ceremony: "Looking at the future availability of oil in terms of world reserves, and currently at the situation in Iran, who can say that the need for this new technology may not emerge sooner than we think?"

'Britain alone cannot end poverty, but it may be able to make a contribution to its end'

Adrian Moyes of Oxfam calls for a transformation of our 'charitable' approach to foreign aid and Third World technology

THERE'S a story of a motorist who stops to ask the way to Andover. The person he asks replies: "If I were going to Andover, I wouldn't start from here." Similarly, anyone trying to help the very poor in the Third World wouldn't start from Britain; most of what needs to be done has to be done by people and governments in the Third World. It is an illusion to think that the wonders of British science and technology can, on their own, do much to benefit the very poor. Whether they do so, depends on the political will in each Third World country.

The use of technology by the very poor has social and political costs. If there are fewer poor people, there may be fewer people prepared to work at low wages for land-owners or manufacturers. Small-scale local production may mean a smaller market for big local manufacturers or multinationals. Urban interests may block investments that will boost agriculture, urban bureaucrats may frustrate policies to direct technology to the poor.

A lot of people benefit from the way that Third World societies are variously arranged at present. They are the people who control the means of production; who own the land or the factories or the transport facilities; who have the money to lend or invest—or to buy modern technology products. They want to keep things that way.

They ensure that there are operating theatres for the townspeople, instead of dispensaries for the rural poor; that R & D is directed to sophisticated or luxury goods and large scale industry; that there is social recognition and praise for research workers who follow international science. They give low priority to extension services and small industry programmes. Foreign investment, some of it from Britain, contributes to this. As *Nature* has put it; "It skews decision-making to favour research and development policies—for example those concentrating on capital rather than labour-intensive production techniques—that benefit the upper and intermediate groups in society, but have little impact on the plight of the poorest".

Technology is a necessary condition for development, but it is not a sufficient one. It cannot get round the need

for political reform, for example by increasing yields so high that even small plots of land become viable and land reform would be unnecessary. Technology is not like that; it cannot solve political problems unless there is political will to solve them.

As a result, most Third World science and technology bears little upon the problems of the very poor. And most scientists and technologists go along with that. They tend to enjoy rather low social prestige (unlike, say, lawyers)—so they turn to the world of international science to get their standing. They go for prestige subjects that will (or at least may) lead to international recognition. Traditional technology and the needs of the poor seldom involve such subjects.

On a personal level too the pressures work against the sort of science and technology that might benefit the poor. Scientists and technologists like the comforts and stimulus of the city; they do not want the inconvenience and discomfort (and perhaps health risks) of spending much time in slums, or in poor, often remote and unpleasant, parts of their country.

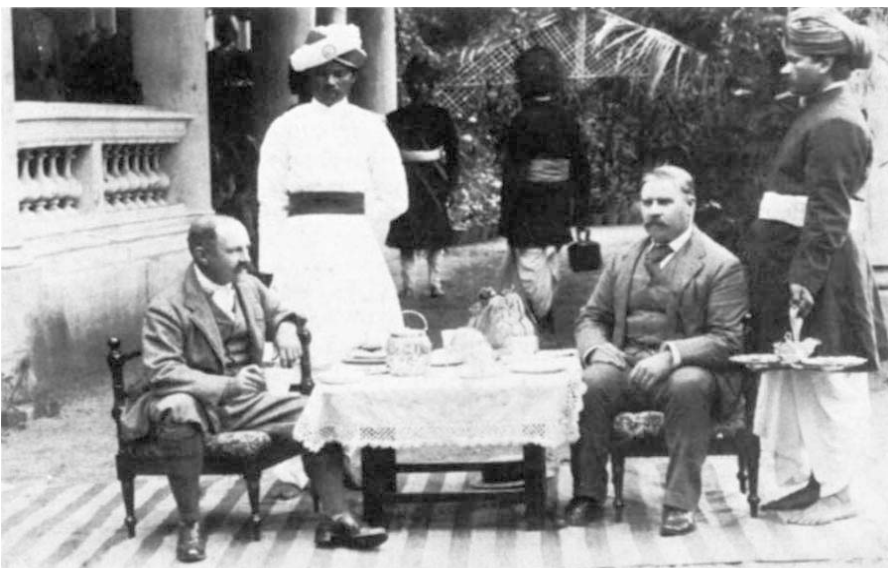
There is not a great deal that outsiders can do about all this. It is not very productive for outside governments to adopt the role of championing the Third World poor against the rich in their own countries.

The difficulties are considerable, but they need not make us despair—Britain alone can't end poverty, but it may be able to make a contribution to its end—it's possible, after all, to get to Andover, even if you start from here.

In the spectrum of technology which runs from the most advanced to the most traditional, Britain's balance of advantage is greatest at the advanced end—so perhaps it is here that Britain can contribute most. Some relatively advanced science and technology can, if social and political conditions allow, benefit the very poor. Examples are education by satellite; the manufacture, storage, and testing of drugs, insecticides, contraceptives; even the equipment needed to devise a better differential for a trishaw; and in the future micro-processors and solar cells. Maybe Britain should concentrate on developing and adapting this type of technology and leave the type of technology that is actually used by the very poor to the Third World countries to handle for themselves.

The drawback to this approach is that left to itself, technology usually favours the rich. It seems to be quite a general rule that if you add to technology to an egalitarian society (and most Third World—and many other—societies are egalitarian) the result is increased inequality. This happens because technology is used primarily by those who can afford to take risks—the very poor can't; for them conservatism is life insurance. The miracle seeds of the Green Revolution benefited mainly the farmers who could afford the fertilisers, the irrigation and the pesticides.

The rich are also favoured because new technology tends to drive out old. In doing so it may deprive some people of their livelihood even though at the



Imperial past: is our technology just colonialism in a modern guise?

same time it benefits many of the very poor by providing cheaper and/or more durable products. The mass of consumers benefit from plastic buckets and plastic sandals, but the producers lose work and maybe jobs.

In the 60s Indian metal-workers began making metal dishes and utensils out of aluminium, instead of the "traditional" but increasingly expensive brass and steel. The plates could easily be cut and shaped with simple machines and tools. They were sold in the villages, often by the workers themselves. It cost about £100 to create one job—an ideal application of appropriate technology it might be thought. But the aluminium came from Birla, India's second largest industrial consortium, and so Birla was alerted to the new market. It began making dishes from anodised aluminium. They sold better because they were shiny. That killed the village version.

So if Britain is to implement its policy of More Help for the Poorest,

it cannot merely concentrate on the sort of technology it is best at—and hope the benefits will trickle down. It must take more direct steps to ensure that the very poor are able to benefit from technology (if they want to).

This requires recognition of the need to strengthen and develop traditional technology, rather than using imported technology to downgrade and maybe destroy it. Traditional technology is much stronger than many scientists and technologists (including Third World ones) think. It includes a wide range of skills (ironwork, waste recycling, hygiene, food preservation), a vast amount of local knowledge, and (provided there is little or no risk) a surprising amount of experimentation (often disparagingly referred to as trial-and-error). The way to help the very poor to make greater use of technology is to help them see how much they know already, and to encourage them to adapt that—and supplement it with new technology

from outside.

A good example of this approach comes from Tanzania where a team spent two months in a village helping the villagers to analyse their own storage problems and devise their own methods of improving them. The team based its activities on the belief that the villagers themselves could go a long way towards solving their own problems on the basis of their own skills and resources. They returned to the villagers in a systematised form all the information they got from them, thus giving them confidence that they possessed a concrete technology.

The villagers were not overwhelmed into accepting the team's suggestions: they rejected many of them, including air-tight stores and communal storage, and picked and chose from along their own practices and the team's suggestions so that the end result was something they had designed themselves.

At the end of the two months, 15

NATURE abhors a vacuum. During the 1939–45 war, urban sites devastated by enemy bombs were rapidly covered with vegetation. The number of species of plants and animals which appeared without any help from man was surprising. Within a few years even trees grew up among the rubble, and provided cover for insects and birds, including rarities like the Black Redstart, several pairs of which nested and reared their young within the boundaries of the City of London.

Scientists made some interesting ecological studies of these sites, and city workers interested in natural history derived pleasure looking at them during their lunch breaks. Economic pressures have caused these oases to be destroyed, to be covered with concrete and asphalt, with buildings and car parks. Opportunities for ecological studies or rural refreshment in our cities has thus been greatly diminished.

Fortunately, however, there have been encouraging developments in the opposite direction. In 1977, in connection with the Queen's Silver Jubilee celebrations, a working party was set up to consider the use of neglected areas of land in London for community purposes. The main drive came from Max Nicholson, for many years the Director General of the British Nature Conservancy. He was chairman of the working party's Environmental Committee. This has several interesting projects in hand in various parts of the city, but the one which appeals to me is the William Curtis Ecological Park in Southwark, near the Tower Bridge and just across the Thames from the Tower of London.

Rus in urbe



KENNETH MELLANBY

Until just over a year ago this three acre site was an unattractive lorry park covered with an uneven surface of hardcore, an unpromising area for ecological study. Most of the hardcore and rubble has been removed, to be replaced with soil. This has been carefully landscaped with undulations and valleys, and a good sized pool, surrounded with marshy ground, has been constructed.

Those managing the site had difficult decisions to make—to plant or not to plant. Wartime experience showed that a good cover of plants would quickly appear without human intervention. However, they wished to encourage native species. Unfortunately disturbed sites in London, as well as in remote oceanic islands like Mauritius, or in projected National Parks in Australia, often attract the wrong species. When visiting this Park recently, I saw that the commonest plants developing on

nearby waste ground were the Oxford Ragwort, a native of Italy, first recorded in England in 1794, and rapidly spread when the railways were constructed in the nineteenth century, and Buddleia, only introduced from China in about 1890, and now a rampant weed in Britain.

The Ecological Park has some areas left for spontaneous regeneration, but most of it is being carefully managed. Many native trees and shrubs have been planted, unfortunately not always successfully as the area has a surprisingly low rainfall and artificial watering is difficult. Wild flower seeds unlikely to arrive unaided have been scattered, and young plants are beginning to appear. The pond is already colonised by insects and invertebrates one might not expect to find in such a location. A kestrel nests in the one remaining large tree, all that is left from the rural countryside built over in the last century.

It will be some years before the park resembles in any way the beautiful fields and woods which once existed nearby, but it already provides unique facilities for local children, brought up in a concrete jungle, to get first hand experience of real ecological problems. Perhaps it will also inspire park authorities in other cities to set up more semi-natural areas to complement their tidy lawns and regimented rows of colourful bedding plants. There have indeed already been some such developments, in Glasgow in Scotland in particular. The virtue of the William Curtis Park is that it shows how much can be done with a small and unpromising parcel of wasteland.

new village-designed grain stores were built and in use, several other improved stores were built, even the poorest were using insecticide, the village storage committee had set up a research project to monitor results, the village had agreed to teach 15 other villages to analyse their storage problems, and the committee held a seminar at the university. "It was at this point, recounts the Report on the project, that the committee members really began to see themselves as storage experts".

These Tanzanians had begun not only a process of improving their storage practice but also a process of using their own and outside technology for their own benefit.

Outside technology however cannot usually be applied directly; its principles and techniques may be valid worldwide, but their use varies as much as the people who use them. When technology is transferred from one society to another, the principles and techniques remain the same, but they are re-arranged and combined with a different set of social and economic activities—to make a technology that has been "transformed".

In order to "transform" a technology, people need a range of possibilities from which they can select and test those most relevant to their needs, combine them with elements from their own technology, and work out the new social and economic arrangements needed.

The new technology may mean more time for women, or more work for

women; more money for everyone, or more money for one or two people; less free time in the festival season, too much work in the peak weeding season; community work to empty latrines, community contributions to pay for a maintenance worker. It may mean new institutions at family level, as when a new agricultural technique tends to transfer work from women to their husbands; at village level, as when village meetings have to choose somebody to go for training as a village health worker; or at local government level, as when the technicians are needed to maintain village pumps, cover a whole district and have to be organised from a central office.

Outsiders (such as people in Britain) can develop appropriate technology—techniques or hardware which perform needed tasks. But only the people who are to use the technology can transform it; only they can adapt their ideas (on the role of women for instance, or the importance of festivals) and their social and economic arrangements.

How then can Britain help the very poor to develop their own technology and transform outside technology? The answer lies in a new sort of programme—a programme whose leading edge is in the slums and villages of the Third World where the poorest are. Field staff, many of them local, would seek out, stimulate and finance projects such as the Tanzanian grain-storage one described earlier, helping it to grow into a countrywide system, including (if need be) training for extensionists and laboratory and workshop facilities.

The field staff would have access to funds not tied to British salaries or equipment, and the programme would be able to finance non-government bodies in the Third World. There are various ways such a programme could be organised—as a development of one of the Ministry of Overseas Development's existing units, as a new Council for the Use of Technology Overseas (or something), or as a non-government body (perhaps a multilateral one).

A programme of this kind would be quite a change from Britain's present aid for science and technology. The basic premise in Britain is that *they* have the problems, *we* have the solutions. Technology made us rich; we have only to apply it to make them rich. The tradition in Britain is to do things *for* the poor in the Third World—a kind of charity even when run by the government.

But it is not much help to make people the passive recipients of a technology designed elsewhere; there needs to be a close connection between the development of a technology and the society in which it is to be used. We should be concerned not so much with the appropriateness of a particular piece of technology, as with the appropriateness of the process of technical change. That is what the new programme would be about. □

This article is based on Good servant, bad master by Adrian Moyes, published on 8 March, available from Oxfam Publications, 274 Banbury Road, Oxford, UK, price £1 post free.

Stockholm institute supports Third World scientists

Dr I. M. Nitis, at the Udayana University of Denpasar in Bali, is working on stylosanthes as a companion crop to cassava. He has discovered that the extra green feed produced by the combination of the two crops could support one or two yearling Bali cattle per hectare for a year. His work has been financed by three grants, ranging from \$555 to \$6,600, from the International Foundation for Science (IFS). "The IFS only gives a little money, but it leaves me to work quite independently", he says. "The beauty of the foundation is that it helps young third-world scientists just starting their own research. If their project is successful, a bigger organisation—the UN Food and Agriculture Organisation, for example—will pick them up." Dr Nitis is one of 250 young scientists in 44 developing countries receiving support.

"Our grants have been limited to research in applied biology and agriculture—work which could be carried out in the villages", explained Professor Sven Brohult, founder and President of IFS. "We want to establish self-

Wendy Barnaby reports on the work of the International Foundation for Science

supporting units on the farms and in the villages. It's like the old Chinese experiment of having everyone self-supporting on a small scale so that nobody actually starves. We must avoid contributing to urbanisation and bureaucracy. We leave big investments to the big organisations. Our idea is to encourage indigenous, simple technology."

So far, grants have been given under six categories: aquaculture; animal production; vegetables, oil seeds and fruits; afforestation problems; fermentation and methods for food preparation; and natural products. "We feel the work has been rather successful in these fields", added Professor Brohult, "and are thinking of going into another: rural construction. On a very simple level, of course. How to construct a fishpond, for example: you have to have a certain slope around the walls.

And we could help in supplying energy to rural communities. Using simple means to dry fish with solar energy."

To qualify for a grant, a project must be relevant to the needs of a developing country and the researchers must be native to, and carry out their work in, a developing country, where their salaries are paid by the institute that normally employs them. All applications are adjudicated by voluntary scientific advisers.

This sort of volunteer work is gratefully received: IFS's total budget last year was just under \$1 million. The money came as government grants from nine countries: Sweden, Canada, France, the Federal Republic of Germany, Belgium, Norway, the Netherlands, Nigeria and Switzerland.

Although the day-to-day running of IFS is left to its small, Stockholm-based staff, guidance and ideas are provided by about 50 member organisations: academies of science around the world. The most recent to join, to Professor Brohult's great delight, is the Royal Society in London. □

correspondence

GMAG: NIH guidelines are not the way

SIR,—Professor Alan Williamson is well within his rights to complain (1 February, page 346) that there was “no real debate” about the proposed basis of GMAG’s new way of working at the public meeting on 21 December, although it is by no means clear at whom the complaint should be directed. By inference, GMAG is somehow to blame. Professor Williamson could just as well have blamed those among his colleagues who chose to use up what time there was in echoing (and amplifying) what the platform had already acknowledged—that there must be special arrangements for some (but not all) homogenic experiments. He might also blame himself for not having brought up the issues to which he now draws attention.

Of these, perhaps the most important is Professor Williamson’s assertion that the UK could with advantage adopt the NIH guidelines and be done with further introspection. It is therefore worth saying that the NIH guidelines are in several respects defective, at least if one supposes that some of the conjectured hazards of genetic manipulation may (or might be) real. First, their classification of animal virus experiments is irrational, and takes no account of conjectured immunological hazards in particular. Second, the potential importance of expression compared with mere replication has been entirely overlooked in the “broadly based public debate” that Professor Williamson admires. Third, even the new NIH guidelines are not constructed in such a way that what must be presumed to be a growing class of experiments without risk can be progressively identified and excluded from rigorous restraint.

I am one of those who believe that if GMAG does not change its procedures soon, we shall find that events force on us a decision between the NIH guidelines and doing the research elsewhere. It is of course ironical that we should be faced with such a choice when the hazards of genetic manipulation remain conjectural. In my opinion, however, it is disingenuous of Professor Williamson and other would-be genetic manipulators to complain at the constraints which they invited a mere four years ago. Making a bonfire of the regulations now will cause only trouble. Surely it is in the interests of the scientific community that they should be dismantled by rational means.

GMAG’s proposed way of working offers precisely such a possibility, provided that genetic manipulators will play their part in gathering the necessary data. My own belief is that if the proposed system were adopted and willingly accepted, we should find that before the end of 1980 all but a handful of experiments were no more restricted than by the requirements of “good microbiological practice”. Is not that a chance worth taking?

Yours faithfully,

JOHN MADDOX
Nuffield Foundation, London, UK



from Nature, 9 November

Risks of recombinant DNA regulations

SIR,—A more appropriate title for your editorial (November 9, page 103) would have been ‘The absence of reason continues’ or ‘Reason ought to prevail’, rather than ‘Now reason can prevail’. Let me explain why I consider the regulations of the recombinant DNA technique and the new GMAG proposal as unreasonable.

● Since the risks of the recombinant DNA technique are principally in the realm of hypothetical scenarios and science fiction, it is totally unreasonable to propose any complex regulations.

● GMAG is proposing to estimate the imaginary risks of various types of experiments, but totally ignores the true risks of regulations themselves. As I have discussed before (*Trends in Biochemical Sciences* 3, NZ43; 1978), the regulations lead to many real dangers to science and society while their benefits are restricted to supporting the livelihood of the bureaucrats who administer the regulatory machinery. It is totally unreasonable to institute regulations without first evaluating the benefit-to-risk ratio of the proposed regulations themselves, and this ratio in the case of recombinant DNA regulations approaches zero.

● As wisely argued by J. D. Watson (*The New Republic* 180, 12; 1979) it is folly to control any human endeavour which may benefit society, just because it is impossible to provide a perfect proof that no risks exist or can be imagined. There is a high probability that such bureaucratic controls will be detrimental to the best interests or society, rather than having any benefits.

The GMAG proposals on the complex numerical estimations of the purely imaginary risks bear a resemblance to the Hans Christian Andersen fable on the ‘Emperor’s New Clothes’ and appear to be a rather pathetic example of where the recombinant DNA folly could lead us. Let’s hope that at the end reason will prevail and all regulations will be converted to a one-sentence statement proposed by D. Stetten, the first chairman of the NIH Recombinant DNA Advisory Committee: “The conditions of containment appropriate for any recombinant DNA experiment are those which are dictated by the most virulent or dangerous organism entering into that experiment”.

Yours faithfully,

McArdle Laboratory for
Cancer Research,
University of Wisconsin, USA

W. SZYBALSKI

Wisdom in universities

SIR,—Your leader ‘Converting intelligence into wisdom’ (25 January, page 251) draws undeniably sound conclusions about our lamentable shortage of far-sighted generalists. But in my view you err in claiming that education must be ‘immensely wasteful’ to be effective. It is simply not true that most of what we learn at school or college helps illuminate future thinking in an indefinable way. Quite the converse: the insistence on specialist teaching, parrot-fashion learning and the unremitting adherence to cumbersome orthodoxies in the academic world are the antithesis of what education ought to mean, and drives out of the minds of all but the most stalwart student any aspiration to true independence or heterodox creativity.

Teaching was originally the responsibility of elders who were accepted as sources of wisdom. It is now a source of profitable employment for those who, all too often, lack the wisdom to do anything better. University teachers are rarely taught to teach. University students who do well in their examinations often look back to realise that they did so in spite of how they were taught, rather than because of it; and few people will argue with the claim that most of what they were taught was distinguished by its irrelevance to conditions in the real world. I have recently been looking into some of the attempts made by universities to get together with industry, for example, in an attempt to make available the wisdom that we like to imagine lurks in the academic fold. In many instances the level of common literacy (to cite just that) is as low in the university-sponsored material as it is in the most backward corner of narrow-minded industry. The inefficiency of universities with regard to staff appointment procedures, administration and communication is remarkable—but is entirely in accord with what I have come to expect.

This is not to say that all university departments are incompetently run. But the very fact that *any* are, undermines what faith society ought to have in the seats of learning. Until there is a thorough reappraisal of what education should mean there is little chance of injecting excitement and liveliness into the academic world. There will be few generalists as long as there is no market for them; and until the unthinkable occurs, and the reappraisal you call for in your leader begins, the opportunities for real progress and revolutionary new insights into man’s predicament will remain unwanted, unfashionable, and frankly embarrassing.

It is, I fear, impossible to upset an apple-cart that is so large, and so weighed down with passengers who are enjoying the apples. It’s fun trying, though.

Yours faithfully,

BRIAN J. FORD
Science Unit, Cardiff, UK.

news and views

Artificial reaction centres to mimic photosynthesis

from Roderick K. Clayton

In photosynthetic tissues, tens or hundreds of molecules of chlorophyll and other pigments cooperate in absorbing light energy that is delivered to a single photochemical machine. The concept of a photosynthetic unit, a photochemical reaction centre together with its share of light harvesting or 'antenna' pigments, was advanced by R. Emerson and W. Arnold in 1932 (*J. gen. Physiol.* **16**, 191) to explain the yield of photosynthetic oxygen evolution by algae exposed to brief, intense flashes of light. It was C. B. van Niel who proposed, on the basis of comparative biochemical studies of bacterial and green plant photosyntheses (see *Adv. Enzymol.* **1**, 263; 1941), that the photochemistry is oxido-reductive: light energy, absorbed by chlorophyll, drives a separation of oxidising and reducing entities. We now know that this photochemical oxido-reduction is the transfer of an electron from chlorophyll (or bacteriochlorophyll, in the case of the photosynthetic bacteria) to an acceptor, often a quinone. Photochemical reaction centres are pigment-protein complexes in which some of the chlorophyll is specialised to react in this way. The reaction centres are embedded in membranes that also carry the antenna pigments and the components needed for electron transport, proton translocation, and ATP formation. Quanta absorbed by the antenna pigments migrate to the reaction centres and initiate photochemistry. The products, oxidised chlorophyll and reduced acceptor, serve as starting points for the secondary events that provide ATP and reduced pyridine nucleotides, which in turn sustain growth.

The first direct sign of photochemistry in reaction centres was the reversible light-induced bleaching of an absorption band near the long wave absorption maximum of chlorophyll or bacteriochlorophyll. This was reported by L. N. M. Duysens using photosynthetic bacteria (bleaching of 'P890'; thesis, State University, Utrecht, The

Netherlands, 1952), and by B. Kok using green plant tissues ('P700'; *Biochim. biophys. Acta* **48**, 527; 1961). The reversible bleaching was a small fraction of the total absorbance, most of which was due to antenna chlorophyll. Reaction centres of photosynthetic bacteria were first isolated as spectrophotometric entities by selective degradation of the antenna bacteriochlorophyll and consequent exposure of the absorption spectrum of the reaction centres (Clayton *Biochim. biophys. Acta* **75**, 312; 1963; Loach *et al. Photochem. Photobiol.* **2**, 443; 1963). They were first isolated as chemical entities through dissolution of the membranes by means of detergents and subsequent fractionation (Reed & Clayton *Biochem. biophys. Res. Commun.* **30**, 471; 1968). Reaction centres have not yet been isolated from green plant tissues for either of the two photosystems. However, the detailed chemical and physical analyses of purified bacterial reaction centres (see *The Photosynthetic Bacteria* (ed. Clayton & Sistrom) Plenum, New York, 1979) have provided insight into the composition and functioning of the reaction centres of photosystem 1 and photosystem 2 of green plants (see *Brookhaven Symp. Biol.* **28**, 1976).

Reaction centres isolated from photosynthetic bacterium *Rhodospseudomonas sphaeroides* are pigment-protein complexes in which four molecules of bacteriochlorophyll, two of bacteriopheophytin (pheophytins) are chlorophylls in which the central magnesium atom has been replaced by two protons), one molecule of ubiquinone and one iron atom are bound non-covalently to a protein composed of three polypeptides, total molecular weight 10^5 . In the first photochemical step following excitation, an electron is displaced from two of the four bacteriochlorophylls, acting cooperatively as electron donor, to one of the two bacteriopheophytins. The electron moves on to ubiquinone in less than a nanosecond. The quantum efficiency of this process is close to 100%. The products, dimeric bacteriochlorophyll cation (B_2^{+}) and anionic ubisemiquinone ($Q^{\cdot-}$), are stable against a back-reaction for about

1/10 in the absence of external electron donors or acceptors. In the living cell these products interact with secondary electron carriers in less than a millisecond.

The elucidation of photochemical mechanisms in reaction centres has been aided by studies of the properties of chlorophylls and pheophytins *in vitro*, and our new knowledge of how reaction centres behave has in turn guided attempts to construct artificial reaction centres that mimic the natural ones in terms of reactions, rates and efficiency. One motive for this is to invent a photovoltaic cell, useful for solar energy conversion, patterned after the natural system. On page 54 of this issue of *Nature* M. J. Pellin *et al.* describe for the first time an artificial system that resembles, in its composition and photochemical properties, a truncated version of the natural photosynthetic reaction centre. In the model system the analogue of bacteriochlorophyll is pyrochlorophyll *a*, chosen for its greater stability against irreversible photo-oxidation. A dimer of pyrochlorophyll *a* is linked to an alcohol derivative of pheophytin *a*, the latter acting as a hydrogen bonding nucleophile. Measurements of flash-induced absorbance changes indicate that electrons are transferred with high quantum efficiency from dimeric pyrochlorophyll to pheophytin in this molecular complex. The photoproduct is formed in less than 6 ps and returns to the neutral ground state in 10-20 ns. This is exactly the behaviour of natural reaction centres from which the ubiquinone has been removed: electron transfer from dimeric bacteriochlorophyll to pheophytin in less than 6 ps, followed by relaxation to neutral ground state bacteriochlorophyll and bacteriopheophytin in 10-20 ns. The success of this experiment suggests that the rapid and efficient operation of photosynthetic reaction centres does not depend to an exquisite degree on the angular and translational coordinates of the components. The artificial system shows promise that more stable variants of the natural system might be developed for solar energy conversion. □

Roderick K. Clayton is Professor of Biology and Biophysics, Division of Biological Sciences, Cornell University, New York.

An expanding Earth?

from W. B. Harland

THE concept of an expanding Earth belongs to this century because until the discovery of radioactivity and its application to geoscience there was no knowledge of an internal source of heat that would prevent the Earth from shrinking. Indeed, until about 1950 most geotectonic debate was between the hypothesis of thermal contraction with a reduction of radius of less than 0.01 mm yr^{-1} and a convection hypothesis for continental drift that assumed a relatively constant radius. The primary features to be accommodated were the orogenic belts with a debatable degree of convergence. So for continental drift it had to be assumed that the oceans must be expanding to maintain a steady-state Earth. The concept of convection with spreading oceans was probably first developed by Osmund Fisher (between 1881 and 1891) but not until 1933 was the concept of an Earth expanding so as to open all the oceans enunciated by Hilgenberg. This was not then taken too seriously as no convincing mechanism was proposed to account for so large a change so suddenly in the later part of Earth history. Indeed it was Carey's detailed and accurate reconstructions of continental drift especially from 1958 onwards that perhaps more than anything else, demonstrated the validity of tectonic evidence to settle the debate active since Wegener's successive formulations (between 1912 and 1928). Strangely, however, Carey's reconstructions required more ocean spreading than could be swallowed up in orogeny and so he wanted continental drift to be largely explained by an expanding Earth.

This recurring question was considered at a recent meeting in London*.

At this meeting S. Warren Carey (University of Tasmania) argued, for example, that the great circle approximately encompassing the Pacific Ocean had expanded substantially with little evidence of contraction along its length. Again, most Earth models postulate a large Tethyan Ocean that successively reduced in size through Palaeozoic and especially Mesozoic time to be finally eliminated; but Carey argued that the Tethys is an artefact formulated only to avoid an expanding Earth and that it had no reality; the same would apply to other postulated earlier oceans. He concluded that the Earth has expanded exponentially, this expansion is recognisable only in the last 200 Ma and the rate has increased to that of the present day $5\text{--}6 \text{ cm yr}^{-1}$. Carey also argued

that the main orogenic belts had begun with equatorial dispositions and that major sinistral shear took place along those lineaments. Such transcurrent juxtaposed features whose dissimilarity had instead suggested to many an oceanic gap.

In the intervening years, from magnetic striping, the course of ocean spreading has been delineated with considerable precision so that latterly the problem was to estimate how far this generation of new crust was compensated by the, much more difficult to estimate, subduction in orogenic belts. H. G. Owen (British Museum (Natural History)) has plotted the striping in a series of detailed maps that also require a more modest, but nevertheless significant, expansion of the Earth during the past 200 Ma at least. Owen's expansion is less than Carey's because he recognises the evidence of subduction zones. It might be added that most plate tectonic reconstructions seem to get by with a steady size Earth but they generally do not plot ocean floor spreading details. In this respect Owen's work is so thorough that it cannot be ignored.

To complete the series one might add a commonly held third hypothesis that the Earth is only slowly expanding (about 0.05 cm yr^{-1} as suggested by Egged, in 1969 and Holmes in 1965) so as not to affect ordinary plate tectonic reconstructions. Such could easily be accounted for by thermal expansion from radioactivity.

Tests of these hypotheses were discussed in general and in fundamental terms by S. K. Runcorn (University of Newcastle) and with rigorous quantitative and geometrical constraints by A. D. Stewart (University of Reading).

The palaeomagnetic test compares the palaeolatitudinal spacing on a continent with that on the present Earth. This assumes an inextensible continent, and a continuing geocentric magnetic dipole and would be capable of settling the question if only the errors were not too large. Results would seem to rule out any extreme expansion but might allow up to 5% over the past 200 Ma.

Earth's moment of inertia would change with increasing radius and there would be a marked slowing with expansion. Estimates of the number of days in a year have been made of around 400 days at about 400 Ma. This would not permit the recent rapid expansion hypothesis; but the critical observations on corals on which these figures are based have yet to be repeated more generally.

The principal mechanism suggested for expansion over and above what secular thermal expansion would effect is that of Dirac's decline of the gravitational constant with time. But that would hardly fit the sudden expansion of radius from 0.6 cm yr^{-1} at 500 Ma and 0.7 cm yr^{-1} at 250 Ma (respectively about 11 and 5% of Earth history and nearer 1% of the age of the Universe). An interesting test of such extreme expansion is provided from extraterrestrial bodies. The smooth lunar maria, formed at about 3.8 Ga, are preserved with no subsequent distortion as would be the case if a cosmic process were to account for a differential expansion of the Earth's crust. An extensile feature on Mars would suggest only a small expansion.

Stewart distinguished planetary interior expansion (perhaps due to decline in G) rupturing the outer shell, from general expansion of the Universe. The latter could not directly produce differential motions such as continental drift but only a change of Earth's radius of 0.5 mm yr^{-1} . A test of palaeogravity change from mineral PT systems does not seem to distinguish expansion models up to 0.5 cm yr^{-1} .

His main contribution was a rigorous quantitative study of the geometrical constraints on the interior expansion hypothesis. On a constant diameter Earth, spreading varies from zero at the poles of relative rotation between two plates, to a maximum 90° away. But on a purely expanding Earth, spreading rates should be essentially constant. It is thus possible to get a measure of the expansion rate by measuring spreading pole-spreading equatorial distance. The result is less than 0.4 cm yr^{-1} over the past 5 Ma. This would eliminate Carey's rapid expansion but not Owen's which is 0.5 cm yr^{-1} average over the past 200 Ma.

Several estimates of subduction rates were used to test the balance of crustal gain and loss. This is simply a return in more sophisticated fashion to the old problem of orogenic shortening. Here there is space only to conclude that the subduction required for a steady state Earth seems to be approximately ($\pm 50\%$) consistent with several methods applied to individual zones, and so at least half of all oceanic crust formed must have been subducted which would rule out the extreme but not Owen's expansion.

Finally Stewart's assessment of the standard palaeomagnetic test from McElhinny's data concludes a rate of expansion of radius only 0.11 cm yr^{-1} which would rule out both Carey's models. This is what most scientists

*A meeting of the Joint Association for Geophysics with the Geological Society of London was held in London on 17 January.

W. B. Harland is in the Department of Geology, University of Cambridge.

have generally concluded though one speaker questioned whether the data-set used to obtain this figure was properly chosen. But the challenge of an expanding Earth will remain until either the tests have been more rigorously repeated or the critical features of Carey's and Owen's models are incorporated in a steady-state model with or without minimal expansion.

Mechanism of platelet–collagen interaction

from J. L. Gordon

THE rapid adhesion of platelets (or of their non-mammalian counterpart, the thrombocytes) to the vascular wall when the endothelial lining is breached has been a subject of interest since Wharton-Jones showed in 1851 that thrombocytes accumulated at points of local damage in blood vessels of the frog's foot. It eventually became clear that the substance that platelets 'recognised' was a component of connective tissue, and of the various components tested only fibrillar collagen induced platelet aggregation *in vitro*. The importance of platelet–collagen interaction in haemostasis and thrombosis was sufficient justification for many research groups to study this phenomenon but its speed and specificity also intrigued some of those with an interest in cell adhesion *per se*, who joined the search for a collagen 'receptor' on platelets and for the determinants on collagen that platelets recognised.

When Jamieson *et al.* (*Nature new Biol.* **234**, 5; 1971) advanced the hypothesis that a glycosyl transferase on the platelet surface mediated adhesion by forming an enzyme–substrate complex with incomplete disaccharide groups on the collagen molecule this provided a great stimulus for research on platelet–collagen interaction. Eventually, the results of further experiments (see for example Menashi *et al.* *Nature* **264**, 670; 1976) made this hypothesis untenable, despite the impressive rear-guard action mounted by its proponents (Jamieson *et al.* *Thrombos. Diath. haemorrh.* **33**, 668; 1975). An important factor in this debate was the discovery that an ordered, fibrillar, quaternary structure was required for collagen to stimulate platelets (for a review see Jaffe in *Platelets in Biology and Patho-*

logy (ed. Gordon) 261, Elsevier, 1976). Thus, in experiments using monomeric collagen (that is, individual molecules composed of three polypeptide chains as a triple helix) where some platelet response to stimulation (for example aggregation or secretion) was measured, collagen polymerisation was a prerequisite for activity. Hence, it was sometimes erroneously concluded that inhibitors were blocking platelet–collagen interaction when in fact they were interfering with collagen fibrillogenesis. This was also exemplified by studies with collagen subtypes which show varying rates of fibril formation: for example, type III is a much more potent stimulant than type I when they are mixed with platelets in monomeric form (Hugues *et al.* *Thrombos. Res.* **9**, 223; 1976) but the potency difference is minimal if preformed fibrils are tested (Barnes *et al.* *Biochem. J.* **160**, 647; 1976).

Even when it was accepted that collagen must be in fibrillar form to stimulate platelets the question still remained whether monomeric collagen (or even fragments of individual molecules) could bind to platelets without inducing aggregation or secretion. Much intellectual and experimental effort was expended on this but the results obtained were often conflicting and the discussions generated more heat than light. One reason for this was that collagen molecules and fragments readily self-associate either when free in solution or during attachment to a matrix such as sepharose beads; hence, varying amounts of pseudofibrils may be present. Platelet–collagen binding experiments are also open to the criticism that weak interactions probably do not survive the separation procedures. The main conclusion to emerge from such studies was that platelets may bind to submolecular fragments of mammalian collagen but, if so, the binding was much weaker than to fibrils. These findings were consistent with the hypothesis advanced by Lüscher *et al.* (*Ser. Haematol.* **VI**, 3, 382; 1973) to explain platelet stimulation by polymers. This introduced the concept of platelets interacting weakly with monomers or fragments through binding sites of low specificity and affinity; when multiple, simultaneous interactions were possible with several sites in optimal alignment (that is, in an ordered polymer with regular repeating sequences) binding was much stronger and a cellular response was induced. Polymerisation alone is not enough—platelets are not stimulated by gelatin (polymerised, but denatured, collagen). It appears that the native fibril formed *in vivo* by collagen type I or type III exhibits an

optimal configuration: platelet aggregation can be induced by $< 1 \mu\text{g ml}^{-1}$ of type I collagen in suspension. In contrast, suspensions of native type IV collagen (which has an amorphous structure) are inactive. However, if collagen types I and IV are dissolved and reassembled as SLS ('segment long spacing') fibrils they are both active platelet stimulants, with an activity threshold around $10 \mu\text{g ml}^{-1}$ (Barnes, Bailey, MacIntyre & Gordon; unpublished work). It therefore seems that the steric arrangement of collagen molecules is much more important than chemical differences between subtypes in regulating interaction with platelets.

The recent paper by Bensusan *et al.* (*Proc. natn. Acad. Sci. U.S.A.* **75**, 5864; 1978) sheds some light on platelet–collagen interaction by concentrating on the platelets rather than the collagen. They induced platelet–collagen adhesion by mixing a suspension of type I native collagen fibrils with well-washed platelets, disrupted the adherent platelets by sonication, and determined what platelet proteins remained adherent. The most important platelet membrane component attached to the collagen was fibronectin, a glycoprotein of $\sim 2 \times 10^5$ molecular weight that is a major component of many cell membranes. Also present were actin, myosin and tropomyosin, which suggested that fibronectin on the platelet membrane was involved in binding to collagen and was also connected to the platelet contractile proteins. The presence of fibronectin was confirmed immunologically, and further evidence for its functional significance was obtained by demonstrating that collagen pretreated with fibronectin did not stimulate platelets, whereas anti-fibronectin antibodies induced platelet secretion. Other points remain to be clarified: for example, plasma fibronectin binds to triple-helical or denatured collagen (Dessau *et al.* *Biochem. J.* **169**, 55; 1978) whereas the isolated platelet protein apparently binds to collagen fibres but not to gelatin—that is, shows the same conformational specificity as for platelet–collagen interaction. Assuming that the findings and interpretations of Bensusan *et al.* are confirmed, however, more emphasis in future considerations of platelet–collagen interaction should be given to the mechanisms involved in fibronectin–collagen interaction. This has great significance in many cell adhesion phenomena apart from those involving platelets (Yamada & Olden *Nature* **275**, 179; 1978). One quibble with Bensusan's paper, terminological rather than technical, concerns the use of the term 'receptor': as usually employed

J. L. Gordon is Principal Scientific Officer in the Cell Biology Department, ARC Institute of Animal Physiology, Babraham.

(for example in the context of cholinergic, aminergic and other sites) this implies a degree of specificity and affinity which is at variance with the characteristics of fibronectin in mediating interaction with collagen. This is, however, a small criticism of a paper that invites a major reassessment of attitudes in this field of research. □

Proposed optical Stern–Gerlach experiment

from Peter Knight

THE atomic beam work of Otto Stern and his colleagues in Hamburg was published in a series of papers bearing the subtitle 'Investigations with the Molecular Beam Method' (*Untersuchungen zur Molekularstrahlmethode*). The thirtieth and last, in 1933 by R. Frisch, reported the observation of the deflection of an atomic beam by the momentum carried by photons (*Z. Physik*, **86**, 42; 1933); it closes with the remark: "It would certainly be possible to do more careful experiments. Unfortunately, for reasons beyond our control, the experiments had to be stopped prematurely". The phenomenon was revived by the work of Ashkin and his coworkers (most recently in *Phys. Rev. Lett.* **41**, 1361; 1978) who demonstrated a large enhancement of the radiation pressure effect using laser radiation. Since then photon deflection has been discussed for atomic trapping and cooling, for spectroscopy and for laser isotope separation (although the latter now seems an outsider in the separation stakes).

A recent paper by R. J. Cook presents a new and quite simple analysis of atomic motion in a resonant laser electromagnetic field (*Phys. Rev. Lett.* **41**, 1788; 1978). He shows how an atomic beam can be split by traversing an amplitude gradient of the resonant radiation in a fashion reminiscent of the old Stern–Gerlach demonstration of space quantisation. This splitting had been discussed earlier by Kazantsev (*Sov. Phys. JETP* **39**, 784; 1974) and aspects of the subject reviewed by Stenholm (*Phys. Rept.* **43**, 151; 1978). Photon deflection is produced by excitation and emission recoils, and by a force caused by the interaction of the induced dipole moment with the amplitude gradient of the radiation field. Cook's paper is concerned with the latter and his results are valid only if spontaneous emission can be neglected during the interaction time.

For an atom driven by a laser such that only two energy levels (with wave func-

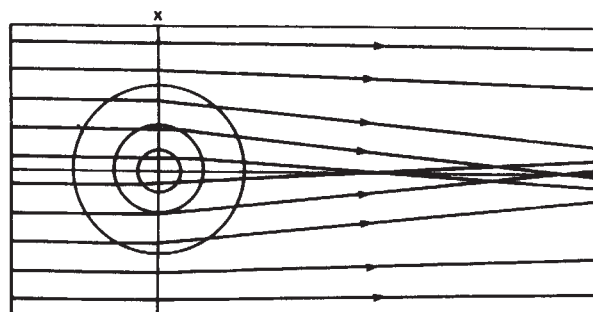


Fig. 1 Focusing of atomic trajectories associated with the wavefunction u_- . (Cook *op. cit.*)

tions c_1 and c_2) are relevant, the analysis is particularly simple. The Schrödinger equation for the time-dependent wavefunction for the atom interacting with an incident field leads directly to two uncoupled equations of motion for the symmetric ($u_- = 2^{-1}(c_1 + c_2)$) and anti-symmetric superposition ($u_+ = 2^{-1}(c_1 - c_2)$) of states. These two combinations represent special 'eigen-waves' for the atomic trajectories which propagate independently. An atom initially in its ground state is equally likely to be in one or the other of these superpositions. The 'eigen-waves' obey similar Schrödinger equations except that the potential energy for the u_+ is of opposite sign to that for u_- . The forces acting on atoms in the two waves are thus in opposite directions so that an amplitude gradient will split a collimated ground-state atomic beam into two components: an optical analogue of the Stern–Gerlach experiment. A well-collimated beam of atoms intersecting a Gaussian laser beam at right angles will see the optical beam as a cylindrical lens which focuses u_- waves but defocuses u_+ waves. (Fig. 1 shows the focused trajectory.)

Cook estimates that a 1 W laser beam focused to a 50 μm beam waist (with waist intensity about 10^4 W cm^{-2}) will act as a lens of focal length approximately 1 cm for an atom of slow thermal speed and typical mass and transition dipole moment. An atomic beam centrally traversing such a cylindrical 'radiation lens' would have a cross-section flux at the focus as shown in Fig. 2 where the u_+ wave has been deflected out of the

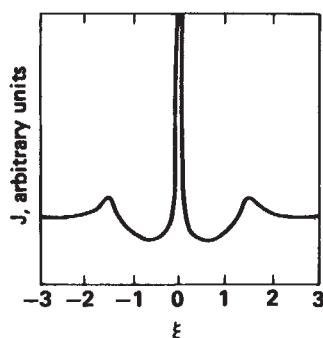


Fig. 2 Total atomic flux J at the focus of the 'radiation lens' for a centrally positioned beam of atoms, in units of laser beam radii (Cook *op. cit.*).

centre and the u_- wave focused.

The lens effect is an interesting demonstration of the inversion of our usual ideas. A lens is normally made of a medium (consisting of polarisable dipole oscillators) shaped so that a collimated light beam is focused by the density gradient of polarisability. Here we have a collimated atomic beam focused (or defocused) by polarisability effects in a light beam shaped to give an electric field gradient. Experimental investigation of these ideas does not seem too difficult and is presumably under way. □

Complexities of cultural evidence in the Lower and Middle Pleistocene

from John A. J. Gowlett

For many years the study of human evolution became, in the words of Irving Hallowell, 'more and more restricted to biological problems dealt with by physical anthropologists'. The cultural evidence, which is essential to a study of the distinctiveness of man, and which is mainly the province of the archaeologist, was scarcely considered as an aspect of biological evolution. The splitting of the study of early man into separate disciplines of archaeology, physical anthropology, and to some extent evolutionary biology, has its roots in the 19th century, and although much has been achieved in the way of interdisciplinary studies since Hallowell was writing, the central importance of the cultural evidence is still sometimes overlooked.

Archaeological work has however built up a record of behavioural complexity of a cultural nature going back at least 2 million years. Man can therefore investigate the evolution of his present cognitive capacity through the permanence of the cultural record. Considering the clear evidence of humanity that is available, it is surprising first that philosophers have made

J. A. J. Gowlett is in the Department of Archaeology, University of Cambridge.

P. Knight is SRC Advanced Fellow in the Department of Physics, Royal Holloway College, University of London.

so little adjustment to this long time-scale, and second that animals other than man are so often used inappropriately as models for human evolution.

Most of the earliest archaeological evidence comes in the form of stone implements, and although these convey much more information when found in context than in isolation, their study is still an important part of Palaeolithic archaeology: they do, after all, fall clearly within the domain of culture, indicating not only the regular transmission of ideas as learned behaviour, but also the 'imposition of arbitrary form on the environment'². In this sense, Holloway has suggested the potential of the artefacts as a behavioural or sociopsychological record, whereas archaeologists have often been more concerned with classification and typology. Even within the Lower Pleistocene period, which as now defined lasts from ~1.8 down to 0.7 Myr ago, stone-working exhibits design regularities, indicating consistent linkages of ideas (and thus allowing typological description: Fig. 1)^{3,4}. In archaeological terminology, the artefacts which a group of prehistoric people 'manufactured in one area over some span of time' form an 'Industry', but in practice the major taxonomic entities of the Lower Palaeolithic are 'Industrial Complexes' or 'Traditions', defined as groupings of Industries 'considered to represent parts of the same whole'⁵. The vagueness of this definition is of limited importance for descriptive purposes as in the Lower Pleistocene only two industrial traditions of stone-working are generally recognised: the Oldowan, known almost entirely from Africa, and the Acheulean, which continues through the Middle Pleistocene, becoming widespread in the Old World.

The Oldowan, which has an approximate age range of 1.8–1.6 Myr at Olduvai Gorge in Tanzania^{3,6}, is characterised by the regular practice of simple flaking, and the production of a range of tool forms, often based on cobbles. Among these, the discoids and choppers show bifacial working: the production of a resolved edge through flaking from both sides, a complex process involving both feed-forward and feedback. Industries from East Turkana⁷ and the Omo⁸, aged about 2 million years, can be ascribed to the Oldowan, but show some differences which may be accounted for partly by differences of raw material. A variant defined as the Developed Oldowan by Mary Leakey succeeds the Oldowan at Olduvai, having a range of ~1.6–1.2 Myr, and reappearing according to her in higher beds dated to ~0.8 Myr. The Developed Oldowan has a slightly wider range of tool forms, with more em-

Typology after M.D. Leakey (1971)		Oldowan	Developed Oldowan A&B	Acheulean	Acheulean typology (E. Africa) after M.R. Kleindienst (1962)	
Bifaces	a,b,c,d		B			Hand-axe
	e, Cleaver					Cleaver
	f,g, Picks					Pick
	'Proto-biface'		A			Knife
Discoid						Disk
Chopper, a-e						Chopper
Polyhedron						Spheroid-Polyhedron
Sub-spheroid						
Spheroid						
Scraper (heavy-duty)						Core scraper
Scraper (light-duty)						Scraper (heavy-duty)
Awl; Burin;						Small flake tools
Outil Écaillé						
Laterally trimmed flakes						Trimmed pieces

Fig. 1 A comparison of commonly used Oldowan and Acheulean typologies, which stresses continuity as well as the addition of new forms. Apart from the bifaces, the two typologies differ mainly in detail of description, designated sub-categories being indicated by lower case letters. Correlations of light-duty tools are approximate.

phasis on bifacial working, including some 'poorly made and generally small bifaes' at Olduvai⁶. The Karari Industry from East Turkana⁹, an industry from Chesowanja in Kenya¹⁰ (both probably aged ~1.5 Myr) and an industry from Gadeb¹¹ in Ethiopia have all been described at least provisionally as variants of the Developed Oldowan, demonstrating the usefulness of this term as a descriptive label.

The Acheulean Tradition continues with most of the Oldowan forms, but introduces a new elaborate conjunction of ideas, in the working of bifaces with a well-defined long axis, made on blanks which are specially struck long

flakes (Fig. 2). The bifacial working clearly was a precondition for this development, but it is not clear whether or not the ideas of a long axis and lateral symmetry preceded the use of long flakes. This point has a relevance to the difficulties which archaeologists find in relating the Developed Oldowan and the Acheulean, which are sometimes contemporary, for the Acheulean is first found within the time range ~1.4–1.3 Myr at Olduvai site EF-HR, and Peninj site RHS, also in Tanzania¹². Few other Lower Pleistocene Acheulean sites are known—among these are the Olduvai Bed IV sites, almost certainly the large site

complex at Kilombe, Kenya¹³, and perhaps Kariandusi—although in the Middle Pleistocene Acheulean sites are very common. The Developed Oldowan and the Acheulean have been interpreted at Olduvai as separate cultural traditions, with the suggestion that different hominid groups were involved as makers. Such ideas have, however, been offered only as possibilities, and with very careful qualification^{3,6}, but the existence of two or more hominid species at least in the earlier Pleistocene now seems incontrovertible. In Olduvai Bed I a robust Australopithecine (*A. boisei*) and *Homo habilis* were both found in association with Oldowan artefacts. From both Olduvai Bed II and from East Turkana there is convincing evidence that *A. boisei* and *H. erectus* existed as contemporaries in the period ~1.5–1.3 Myr. At Chesowanja, finds made in 1969 of *A. boisei* were close to recent excavations where artefacts have been found *in situ*¹⁴.

At present neither the palaeontological nor archaeological evidence seems likely to show whether in fact more than one hominid species made stone tools, but Holloway's work on cranial endocasts of fossil hominids does indicate that the brain organisation of various Plio-Pleistocene hominids, including the Australopithecines, was basically human-like rather than ape-like¹⁵. As other assessments of what is 'advanced' in a hominid are largely subjective, it must be recognised that we have no *a priori* grounds for saying that any of these hominids did not make artefacts.

This issue is an important one in the study of human evolution, and needs to be faced openly, but it is not necessary to demand immediate answers, and indeed the growing emphasis on contextual studies encourages other lines of approach. It may be observed that the current definition of 'Industrial Complex' allows use either in a simple descriptive sense, or in a more specific sense of cultural entity, whereas the artefact record in the Lower and Middle Pleistocene, seen in general, shows much continuity of ideas (Fig. 1) coupled with much variability in detail. Some of this variability, which occurs in size, morphology, and proportional frequencies, can be explained by factors such as (1) short-term stylistic change; (2) activity variants, where a human group might use specialised tool kits for individual tasks¹⁶; (3) raw materials, the form and fracture properties of which might influence the end-product; and (4) random or stochastic variation¹⁷. In contrast, remarkably high levels of standardisation are sometimes achieved at a particular locality, implying strict controls. Such combinations of flexibility and stability may reflect the essential adaptive

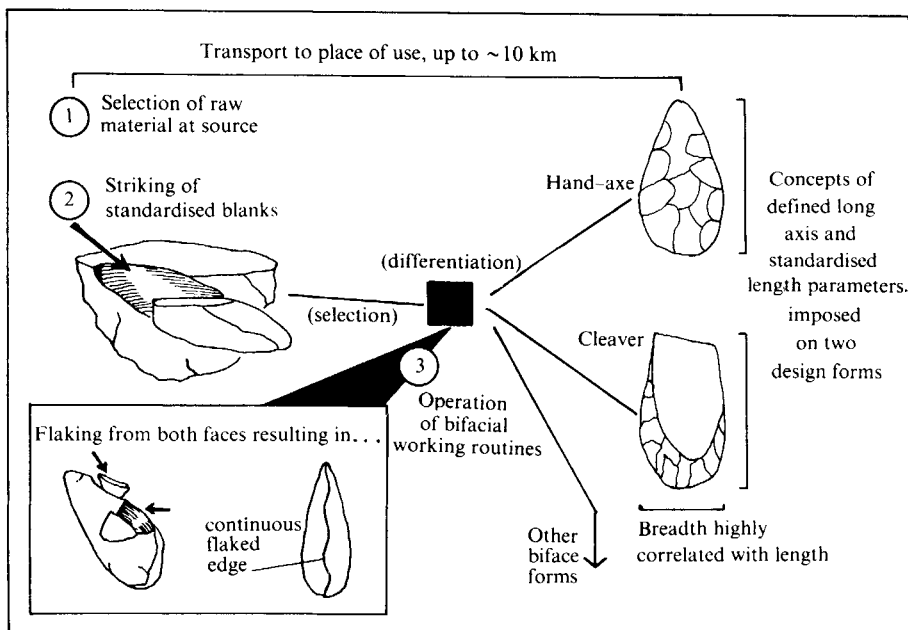


Fig. 2 A simplified diagram of the complex pattern of biface manufacture encountered on Lower and Middle Pleistocene Acheulean sites. Operated as a multi-stage process with geographic separation of stages, it is powerful evidence for organised forward planning, and metrical data provide evidence for remarkable levels of control.

advantages of culturally controlled behaviour. Terms like Oldowan and Acheulean lay stress on selected continuities, and unless used purely descriptively may eventually prove too rigid to embrace the complexities of the archaeological material.

The contextual evidence helps to illustrate this complexity. Even in the Oldowan, artefacts were transported to site, sometimes several kilometres from the source rock, and careful selection of raw material is apparent. In the Acheulean the process is much more complicated and given the distances of transport, and quantities of material, it is interesting for what it can tell us about goal-seeking behaviour, forward planning, habitual and systematic exploitation of an environment, and cognitive mapping of that environment. A great deal can thus be gained from the study of sites in relation to the sediments that contain them. In general sampling density is very low because of the great timespans, and coupled with the high sample variability presents 'in methodological terms... a tough situation'¹⁸. Isaac has treated the background to this in papers which make an important contribution to theory¹⁸. Certain exceptional experimental circumstances, with for example high densities of sites in continuous sediments (such as Olorgesailie or Kilombe), or with sediments extensive over many square kilometres (such as at East Turkana)¹⁹ allow tests which may give some firm answers about the nature of variability in controlled spans of space and time.

In spite of limitations imposed through poor time calibration within sequences, and biases from non-random patterns of exposure, such experiments can work on the density and spatial distribution of occupation traces, and when linked with studies of the transport of raw materials, suggests a whole range of research possibilities touching on factors of interest such as group size, territoriality, and localised or seasonal adaptations.

To further such work, much more archaeological fieldwork is needed, as the corpus of 'routine' finds is very small by the standards of other areas of archaeology, and as only through such work will exceptional finds of unexpected nature crop up from time to time.

It is, unfortunately, too easy for a dogma to arise about future research objectives and methods of working. In the past few years, the exploration of certain extensive areas of sediments, belonging especially to the time-span 3–1 Myr, has dictated the terms of 'palaeoanthropological' research, and led to a particular interdisciplinary balance, which relates mainly to work in the field. Judging from past experience, it cannot be assumed that these approaches will be maintained unaltered in the future. The 'environmental paradigm' though indispensable, forms only one segment in the possible range of approaches: in human evolution, the study of human intellectual development is clearly no less important than that of 'origins', and the potential for cooperation between, for

example, workers in archaeology, hominid neurology, and psychology, is already becoming apparent, although it can hardly happen in the field. To set this subject matter within the context of work in the philosophy of science is also desirable, for the archaeological record has a potential for broadening time-perspectives, and perhaps significant contributions to make towards studies of the phenomenon of consciousness or self-awareness.

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Relativistic nuclear collisions

from A. M. Poskanzer

In the past few years the Bevalac Accelerator at Berkeley has allowed the study of collisions of nuclei at energies which before were accessible only with cosmic rays. The maximum energy attainable, 2 GeV per nucleon, is two orders of magnitude higher than was previously available in the laboratory. At present, beams as heavy as ^{40}Ar are in use, but a 2-year improvement project has started which will allow acceleration of beams up to uranium. The construction of similar relativistic heavy ion accelerators has been proposed in West Germany, Russia, and Japan.

The main goal of studies of relativistic nuclear collisions is to make nuclear matter at high density and temperature, with the aim of studying the equation of state of nuclear matter. A sketch of the theoretical binding energy of nuclear matter as a function of density is shown in the figure; the derivative of this curve with respect to density is the equation of state. What is known experimentally about nuclear matter at present is one point on the graph; the equilibrium density of nuclei, and the binding energy of nuclear matter at this density. Also, we know that at this point there is a minimum, which is to say, nuclei are bound. Even the curvature at this

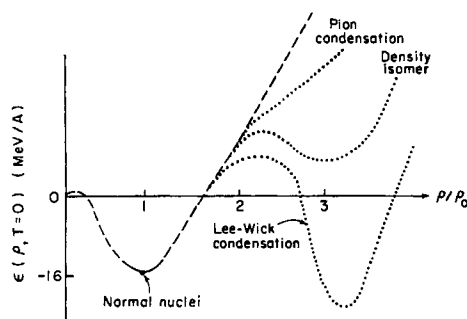


Fig. 1 (From Poskanzer *Comm. Nucl. & Particle Phys.* **7**, 41; 1977).

point, that is, the compressibility, is poorly known experimentally. The various curves are some of the speculations about what might happen at high density. At still higher densities it is possible that the nucleons might break up into their constituents to produce quark matter.

It is clear that the first requirement for making nuclear matter at high density and temperature is to collide a high energy heavy projectile at small impact parameter on a heavy target. Goldhaber (*Nature* **275**, 114; 1978) estimates that densities of four to ten times normal nuclear density could be achieved in collisions on uranium nuclei with uranium ions of 2 to 8 GeV per nucleon. Thus, in a system containing almost 500 nucleons, one may produce both matter and energy

densities far in excess of any previously attained in a volume of nuclear size.

However, research proceeds in smaller steps for practical reasons and the first results on central collisions were for 250 and 400 MeV per nucleon ^{20}Ne ions on a uranium target (Gosset *et al. Phys. Rev. C* **16**, 629; 1977). These results, on the energy spectra of protons and light nuclei emitted in the reactions, generated enormous interest, and by now have been fitted by 20 groups of researchers using 12 distinct models. The extreme disparity in the bases of these models is best typified by comparing the fireball model, a so-called thermal model, to the intra-nuclear cascade model. In the nuclear fireball model one assumes a clean-cut geometry which divides the nucleons into spectators and participants, and then that the participants achieve thermal equilibrium before separating. In the cascade model the two-body collisions of all the constituents are followed. The aim in these model calculations has been to describe the 'background' physics of what goes on, so that one could search for interesting new effects. In the metaphor of the Berkeley Summer Studies, the models are tools for cutting through the weeds to look for the flowers. The amazing thing is that, within factors of two, most of the models were able to fit the data. It was thought that energy and momentum conservation together with a spectator-participant concept, constrains the models so that differentiating between them must await more accurate and extensive data. However, let us examine another possible explanation.

The most recent publication of a calculation is by J. D. Stevenson (*Phys. Rev. Lett.* **41**, 1702; 1978). It contains results from one of the five intranuclear cascade calculations which are in progress. Because of the reasonable fit obtained he concludes that "this calculation shows that the radical assumption that a hot nuclear fireball is formed in nucleus-nucleus collisions is not necessary to explain existing experimental results." On the other hand, he also shows that the average emitted nucleon has undergone five to six scatterings and quotes results from gas studies to the effect that this is more than enough to show equilibrium features. Thus, it appears that Stevenson has shown microscopically that there is some justification to the macroscopic thermal models. He has possibly also given another explanation of why most of the model calculations agree: the large degree of thermalisation obscures the initial assumptions of the models. The significance of this point buried in Stevenson's article is not trivial. Although one can follow collective motion microscopically, macroscopic models are enormously simpler, and

A. M. Poskanzer is Scientific Director of the Bevalac at the Lawrence Berkeley Laboratory, Berkeley, California.

much more easily relate effects to observations. In addition, all the interesting effects we are looking for are macroscopic effects. Classical microscopic calculations, like the one by Stevenson, are useful in testing model assumptions, such as the degree of thermal equilibration, but provide little insight into the collective properties of hundreds of nucleons. We need macroscopic models characterised by a few bulk parameters, such as the compressibility modulus, which are adjusted to experimental data in order to learn new physics. For instance, for compression and re-expansion effects, some macroscopic calculations have just recently pointed out the effects which might be seen experimentally.

The data fitted by Stevenson were taken with a detector of small solid angle so that the experiment looked at only one particle from each event but accumulated data for many events. However, present experiments measure the multiplicity of charged particles associated with the particle in the main detector, and thus are able to select central collisions on the basis of their high multiplicities. Experiments that measure two particles in each event are in their early stages. These two-particle correlation studies will give a much better indication of the degree of thermalisation, because in a single nucleon-nucleon scattering the two nucleons tend to be correlated at 180° in the centre of mass, while in a thermal model they would be uncorrelated. The future lies in measuring as many of the final particles as possible in each event and thus determining the collective motion of the nucleons emitted. One hopes that one day it will be possible to relate data of this kind back to the equation of state of the compressed nuclear matter. $\square$

Insects need cilia

from Michael Sleight

It is generally agreed that arthropods lack water-propelling ciliary organelles of the 9+2 pattern, except on the sperm of some members of the group. Recent observations by Bradley and Satir (*J. Cell Sci.* **35**, 165; 1979) suggest that insects have found it necessary to develop alternative structures to perform the function of fluid propulsion or at least the transport of uric acid crystals, in the Malpighian tubules. The authors have named these structures axopods, by analogy with the stiff microtubule-supported pseudopodia of certain amoeboid protozoans, the Heliozoa and Radiolaria.

The axopods found in the lumen of

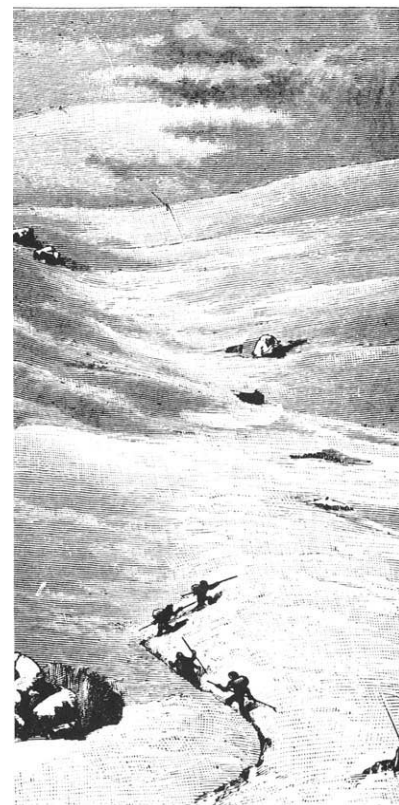
Malpighian tubules of the bug *Rhodnius* are branching projections occurring among, but longer than, the microvilli of the cells lining the lower tubule; they contain between 1 and about 50 microtubules and vary in thickness between about 0.2 and $0.8\ \mu\text{m}$. The microtubules are separate and regularly spaced, several diameters apart, but without any of the specific patterns or links found in protozoan axopods (Cachon & Cachon *Ann. Biol.* **13**, 523; 1974). Where the insect axopods branch, groups of microtubules separate and pass into each section. At the base of the axopod the microtubules extend deep into the cell to end near an inner cell boundary or in a cluster of mitochondria; no association has been found with a centriole or other microtubule-organising centre. Sections of the axopods reveal groups of longitudinal microfilaments close to the peripheral membrane.

It has not been possible to gain a clear view of the insect axopods in motion, but the movement of uric-acid crystals is consistent with propulsion by the axopods, and no other means of propulsion appear to be present in the experimental situation. The propulsive action is assumed to take the form of active lashing of the axopods. Analogous microtubular organelles other than cilia and flagella do not normally show this form of motion; the straight axopods of Heliozoa merely change their length to promote cell movement (Waters *J. Cell Sci.* **3**, 231; 1968) and the haptonemata of certain flagellates, flagellum-like projections that contain a small number of single microtubules, perform coiling movements (Leadbeater *J. mar. biol. Ass. U.K.* **51**, 207; 1971). The appendages of the marine protozoan *Sticholonche* may come nearest to insect axopods, for the numerous, stiff, microtubular exopods of *Sticholonche* are pivoted at the base and are caused to perform rowing motions by the contraction of (non-actin) microfilaments attached to their bases (Cachon, Cachon, Tilney & Tilney, *J. Cell Biol.* **72**, 314; 1977). It seems likely that microvilli of insect malpighian tubules may have been invaded by microtubules; while these provide the stiffness necessary for the propulsion of fluid, the actual bending forces may be provided by microfilaments, as Bradley and Satir suggest. Fluid propulsion of a similar type has been observed in a number of other situations in arthropods; it will be interesting if similar forms of modified microvilli have been adopted elsewhere or whether true cilia do exist on epithelial cells of arthropods. $\square$

Michael Sleight is Professor of Biology at the University of Southampton.



A hundred years ago



Our readers may remember that early in the year General de Nansouty, the hardy director of the Pic du Midi Meteorological Observatory, was cut off from communication with the world below, the severe weather having so affected the telegraph as to prevent it from acting. Fears were entertained for the General's safety, and M. Albert Tissandier resolved to organise a party for the ascent of the Pic and the succour of the veteran observer. An interesting account of this ascent appears in *La Nature*, to which we are indebted for the accompanying illustrations. The snow-storm having somewhat abated at Bagnères-de-Bigorre on January 9, M. Tissandier resolved to attempt the Pic next day, in company with three of General Nansouty's usual guides.

They set out at 9 A.M. on the 10th, and in spite of the deep snow and fallen avalanches, the ascent was at first not difficult. After equipping themselves for snow work at some huts occupied only in summer, the ascent was begun in earnest. The weather was grey and uncertain, the temperature 0°C , with a thick mist that prevented anything being seen beyond 300 metres. The snow became deeper and deeper as they advanced, and one of the guides went before to show the way, the others following the marks of his footsteps up the steep slope of the mountain side. Sometimes they were buried to the waist, and often they had to rest to recover breath. From *Nature* **19**, 27 February, 390; 1879.

review article

Nerves and genes

William G. Quinn & James L. Gould

Department of Biology, Princeton University, Princeton, New Jersey 08540

Work with single-gene mutations in a variety of organisms is yielding insights on how genes build nerve cells and specify the neural circuits which underlie behaviour.

It is the business of scientists to explain away the magic in the world. The largest coherent body of magic remaining is the behaviour of men and animals. Behavioural geneticists try to construct plausible explanations for behaviour patterns by studying genetic variants. Historically, they have sought to compare inbred strains, or to cross them and examine the progeny¹. Such comparisons inevitably involve many genetic differences, making the results virtually impossible to interpret. A more recent approach initiated by Delbruck and Benzer has been to generate single-gene mutations which affect behaviour, to analyse the mutants, and to try to infer general principles about how normal behaviour is created. One advantage of such single-gene changes is that now behavioural patterns can be dissected into their component parts. This genetic extension of traditional ethology should have something important to say about how behaviour is organised and regulated, and has been reviewed recently². A more ambitious and potentially more powerful strategy is to examine the effects of single-gene mutations on the ultimate common denominator of behaviour—the physiology of nerve cells and the wiring patterns of neural circuits. This review traces the recent progress of this approach, selecting the work which seems to offer the best prospects for the future.

Single cell physiology

The underlying mechanism for neural activity is ionic. Typically, a metabolically dependent ion pump moves Na^+ out of and K^+ into the neurone, creating a steady potential difference across the membrane. In an action potential, depolarising the cell membrane beyond a threshold voltage opens channels in the membrane specific to each ion, the sodium channel opening and closing more rapidly than the potassium channel. The resulting large change in membrane potential depolarises nearby patches of membrane, allowing the propagation of the impulse through the cell. When the action potential reaches a presynaptic terminal, Ca^{2+} enters through its own channels, causing the release of chemical transmitters which diffuse across the synaptic junction to the next cell, opening still another class of ion channels in the next cell, and thus continuing transmission of the electrochemical signal.

The elucidation of this picture, and the explanation of most of the cell's electrical properties in terms of a few ionic conductance changes, is the major triumph of neurobiology in the last half-century. A substantial problem remaining for neurobiologists is to understand the molecular operation of the various ion channels (presumably proteins) in the neuronal membrane. Two organisms have been found in which the channels can be altered by mutation.

Paramecium

Although unicellular, *Paramecium* behaves as if it were rational. Ordinarily, it propels itself forward through the water by the coordinated beating of cilia distributed over its exterior. However, if it swims into a physical barrier or a noxious chemical, it quickly reverses the direction of its ciliary beat, swims backwards for a time, then sets off in a new direction³. The simultaneous reversal of all the cilia suggests an electrical basis for the behaviour, and experiment bears this out. It is possible to record intracellularly from *Paramecium*, and observe an extraordinary repertoire of ion conductance changes⁴. These can best be understood by following events during an avoidance reaction.

Mechanically stimulating the front of the animal (mimicking collision with a barrier) depolarises the membrane, probably by local influx of sodium ions. This depolarisation spreads throughout the cell by passive current flow and raises the membrane potential above threshold. At this point, a separate, calcium conductance channel is activated, and several regenerative action potentials occur, based on calcium influx. The increased Ca^{2+} concentration inside the cell somehow changes the pattern of ciliary beating⁵, reversing the direction of power strokes so the *Paramecium* backs up. After 1–5 s, the calcium level in the cell decreases (presumably by active ion pumping) and the animal resumes forward swimming.

This fixed action pattern involves relatively few events, and at least two of them are ion conductance changes. Therefore, one can screen for behavioural mutants in *Paramecium* with the hope that some will have altered channels. Using ingenious selection techniques, Kung *et al.* have found many abnormal behaviour patterns⁶. Two are especially dramatic: *pawn* mutants, like chess pawns, cannot back up. This is because their membranes will not produce calcium spikes even when depolarised far above normal threshold. Apparently, *pawn* mutants lack the normal voltage-dependent calcium conductance channels. The *paranoiac* mutant, on the other hand, shows bouts of reverse swimming whether it encounters noxious stimuli or not. Electrophysiology shows that *paranoiac* mutants have normal sensory generator potential and calcium spike initiation, but are blocked in membrane repolarisation after the action potential⁷. Instead of a series of spikes, they show a long, flat peak. This abnormality of *paranoiac* is expressed only in the presence of external sodium⁸. The membrane component affected by *paranoiac* mutations remains unknown. However, radioactive tracer studies indicate that the depolarised membrane in the mutant becomes abnormally conductive to many ions, including calcium, which explains the long periods of ciliary reversal⁹.

So far so good. The visible behaviour of *Paramecium* is closely correlated with the electrical properties of its membrane, and some behavioural mutations affect ion conductances in an apparently straightforward manner. Sophisticated electrophysiological experiments are feasible, such as clamping the membrane voltage quickly to a new level and watching the kinetics of conductance changes. Oertel *et al.*¹⁰ built such a voltage clamp and compared *pawn* mutants (which have no calcium spikes) with normal *paramecia*; thus, they were able to measure the conductance changes specifically attributable to the calcium channels. One could hope to do similar voltage clamp studies with 'leaky' *pawn* mutations which do not completely abolish calcium conductance, compare the kinetics of calcium conductance changes in normal and mutant individuals, and try to gain some insight into the mechanism of voltage-dependent channel activation and inactivation (gating).

Prospects for chemically isolating the channel proteins also look good. The lipid membrane which sheaths each cilium is continuous with the plasma membrane of the cell. When treated with agents such as chloral hydrate, the cilia become stiff and break off from the cells; they can be isolated and membrane fractions purified from them using conventional biochemical methods¹¹. The calcium channels, at least, are concentrated in the ciliary membrane (de-ciliated *paramecia* show no calcium spikes^{12,13}). This raises the possibility of finding altered membrane proteins in *pawn* mutants, then examining the chemical alterations in the mutant channel. Explaining ion channels and gating in chemical terms is a serious problem; *Paramecium* seems a logical place to try.

However, the behaviour of *Paramecium* is much more complicated than the outline above suggests. An increasing variety of unexplained behavioural mutants (spinners, slug-gishes, staccatos) have been isolated. Detailed electrophysiology shows a correspondingly bewildering array of ion conductance channels in the cell membrane. It is as if *Paramecium* tried to compact all the functions of a nervous system into one cell.

These eccentricities of *Paramecium* do not detract from its real promise. Voltage-dependent calcium channels are present in the ciliary membrane. It is a matter of time before the channel proteins, apparently specified by the *pawn* genes, are chemically isolated. *Paramecium* is the one system where electrophysiology, membrane chemistry and genetics can all be directly applied to find out what a conductance channel looks like and how it opens and closes.

Drosophila

At first glance the fruit fly seems unpromising for neurophysiological studies: most of its 10^5 neurones are very small ($<5\ \mu\text{m}$); they interact in a tangle of neuropil, and they receive their oxygen supply through tracheoles, making respiration of the preparation a problem. These difficulties limited the success of the earliest studies on adult mutants by Ikeda *et al.*¹⁴⁻¹⁷. They were overcome by the inspired naiveté of L. Y. Jan, Y. N. Jan and W. A. Harris, who simply slit third-instar larvae down their backs and pinned them out with the nervous system and ventral muscles exposed. The muscle fibres in two longitudinal cords are wide, flat and easy to record from, and the motor nerves can be cut and stimulated directly with a suction electrode. Using this system, Jan and Jan have worked out the bases of the muscle resting and synaptic reversal potentials, identified L-glutamate as the probable transmitter^{18,19}, and shown that in low calcium solutions neuromuscular transmission in *Drosophila*, like that of the frog²⁰, is quantal, each unit of postsynaptic excitation presumably being caused by the release of transmitter from one synaptic vesicle. Thus, given a mutation affecting synaptic transmission, they could determine if the abnormality was presynaptic (affecting the number of quanta released) or postsynaptic (affecting responsiveness to transmitter and hence the apparent size of quanta). Such analysis also allows the detailed kinetics of transmitter release to be followed.

Screening of previously isolated mutations which affect whole-fly behaviour revealed one in which the larval neuromuscular preparation was also altered. Flies carrying a *shaker* mutation²¹ show spontaneous rhythmic leg movement under anaesthesia. Larval neuromuscular transmission is relatively normal in physiological conditions but becomes obviously different in low calcium solution. Paradoxically, it shows greater synaptic efficacy, with more transmitter released over a longer time course. Jan *et al.*²², applying classical physiological techniques, showed that the abnormality in *shaker* is presynaptic. Conductance to calcium was prolonged up to 1 s after stimulation of the mutant nerve terminal. Apparently, the calcium channels in the mutant open normally but are slow to close, so that calcium continues to enter the presynaptic terminal, causing greatly increased vesicle fusion and transmitter release.

It is then natural to wonder whether the abnormality is in the calcium channels themselves or in the action potential which causes them to open. In fact, the action potential is abnormal in the mutant. Normal synapses can be made to behave like *shaker* mutant synapses by applying tetraethylammonium or 4-aminopyridine, drugs which block potassium conductance. Furthermore, extracellular recording shows electrical activity in the mutant motor nerve which persists after stimulation has ceased (Jan, Jan and Wu, unpublished data). All this indicates that the *shaker* mutation blocks potassium channels in the motor neurone, so the voltage-dependent increase in potassium conductance which repolarises the nerve after an action potential²³ never occurs. Extended depolarisation of the nerve then leads to prolonged calcium conductance and enhanced vesicle release at the terminals.

This experimental success stimulated a direct search in Benzer's laboratory for new physiological mutations, specifically temperature-sensitive paralytics which affect larvae as well as adults. One such mutant, *nap*, was found²⁴. Its name describes both its outward appearance (torpor) and its physiological abnormality (no action potential) at 35 °C. Motor nerves in mutant larvae fail to conduct action potentials at this elevated temperature, whereas those of normal larvae are unaffected. Most probably, the mutant gene specifies voltage-dependent sodium channels, which become blocked at high temperature.

If the above arguments are true, then mutants are available in *Drosophila* which affect the two conductances classically defined by Hodgkin and Huxley: *shaker* blocks g_K while *nap* blocks g_{Na} at high temperature. We are left with the problem of what to do with the mutants. It would be impossible to voltage clamp a mutant motor nerve to measure conductance kinetics directly, although it may be worthwhile to search for larger neurones in the fly which are similarly affected. Prospects for biochemical studies with the mutants also look intimidating. Nevertheless, if one set out to apply genetics to the phenomena underlying the Hodgkin-Huxley model—ion selectivity and voltage-dependent conductance changes—one could not hope for a better start.

A less easily explained physiological mutant, the *bang sensitive* (*bas*) fly, has been found in several laboratories^{25,26}. These mutants pass out for several seconds after mechanical shock, as when their culture vials are rapped on a table. Jan and Jan²⁷ discovered that one of these, *bas*^{MW1}, is specifically enhanced in long-term facilitation at the larval neuromuscular junction. Long-term facilitation, sometimes called post-tetanic potentiation, means increased synaptic efficacy after a train of repetitive stimuli. It is phenomenologically similar to ordinary (short-term) facilitation, except that the amount of pretreatment stimulation required for its onset is greater and it lasts longer. The two processes can now be formally distinguished using mutations: *bas*^{MW1} enhances only long-term facilitation, whereas *shaker* specifically increases short-term facilitation. In *bas*^{MW1}, long-term facilitation requires sodium (but not calcium) influx to the presynaptic terminal during the pretreatment period. The increased synaptic efficacy after pretreatment is due to a prolonged period of calcium conductance in the terminal after each stimulus. How a rise in intracellular sodium might

induce the calcium gates to remain open is not understood. Whether other ion channels are affected is not known.

Normal flies (and other invertebrates²⁸) show long-term facilitation like *bas*^{MW1}, although they require longer and more intense pretreatment stimulation. Also, the physiological properties of *bas*^{MW1} can be mimicked by treating a normal neuromuscular preparation with ouabain, an inhibitor of the sodium-potassium pump. This suggests that the *bas*^{MW1} mutation may simply reduce the efficiency of the pump. Alternatively, it could accentuate the mysterious process by which intracellular sodium leads to facilitation.

Neural circuits

The behaviour of multicellular animals is the result of interactions between nerves. Conceptually at least, the question of how nerves combine to produce behaviour may be broken down into two parts: how nerve circuits work, and how these circuits come to be appropriately wired during development. In reality, however, the study of circuitry inevitably leads to questions of neural development and specificity. This is because mutations affecting wiring usually begin to act during development. As a result, genetic studies of nerve interactions which have proceeded beyond the level of drawing the circuit diagrams—no mean task in itself—have either evolved into studies of development or abandoned the 'genetic lesion' technique for the laser-beam scalpel, or both.

Mapping *per se* is an attempt to define which cells are connected to which, and to characterise the anatomical interactions as strong or weak, electrical or chemical, inhibitory or excitatory, and so on. Direct physiological confirmation of these descriptions has rarely been possible, however, for reasons encapsulated in Gould's corollary of Quinn's law. Quinn's law states that the convenience of genetics multiplied by the quality of the behavioural repertoire is a constant for all animals. Hence, bacteria have superb genetics and marginal behaviour, while undergraduates have very interesting behaviour but cumbersome genetics. Gould's corollary states that the quality of the genetics multiplied by the ease with which single nerve cells can be identified and penetrated is also a constant.

However, mapping of isogenic organisms has produced one very encouraging general result: the overall pattern of wiring—which nerve cells are connected to which—is found to be remarkably constant from one animal to the next, whereas the details of the wiring—the specific number and anatomical location of the connections—are quite variable. As the same genes produce the same behaviour against a background of synaptic and dendritic variability, the hyper-fine structure of neural wiring can reasonably (and fortunately) be ignored²⁹.

Nematode nervous system

The most ambitious attack on the genetic control of neural wiring is being made in Brenner's laboratory with the nematode *C. elegans*. This millimetre-long creature has 'good' genetics³⁰, compensatingly simple taxis behaviour^{31–33}, and a seductively finite 258 nerve cells. Intuitively, it seems possible to map all the nerves and look for general principles of circuitry. Goldschmidt had the same idea 70 years ago, and mapped the large nematode *Ascaris* using serial sections and a light microscope³⁴. Brenner *et al.* have mapped most of *C. elegans* using an electron microscope, 20,000 serial sections per nematode, a computer, and much persistence^{35,36}. In nearly all respects, *C. elegans* is found to be a miniature *Ascaris*.

Conceptually, the nematode nervous system is divisible into four parts: the head, which contains virtually all the sensory apparatus; the pharynx, which mediates ingestion; the nerve ring or 'brain' in the neck; and everything further back. The muscle system can be classified along the same lines: the head muscles, which aim the head; the pharynx muscles, which manipulate and pump food; the neck muscles, which help aim the head and aid in swimming; and the rest of the body, which swims.

The 50 or so nerves in the head have been mapped and guesses have been made about their functions. Specific chemo-taxis and tactile mutants have been created which display variable anatomical aberrations at least roughly corresponding to these guesses²⁵. However, the incomplete penetrance and pleiotropic nature of these sensory mutations is frustrating.

There are about 20 nerves in the pharynx—sensory cells, interneurons, motor neurons, and multi-functional cells which seem to act as both mechanoreceptors and motor neurons. These have been sorted into a hypothetical but plausible circuit with typical ethological releaser and fixed-action pattern units³⁷. The present impossibility of doing electrophysiology on this animal and the lethal nature of defects in a feeding circuit make analysis difficult.

The 200 or so nerves which interact in the nerve ring create a maze so complex that little can be said about them except that the sensory nerves from the head as well as the head and neck muscles go into it, and the interneurons which control the ventral cord come out of it³⁸.

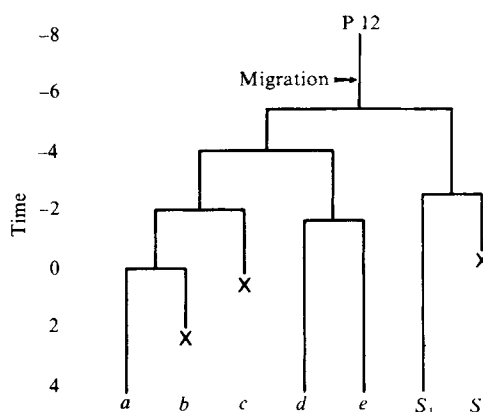


Fig. 1 Schematic representation of post-embryonic development of cells in a portion of the nematode ventral nerve cord, showing patterned divisions from a single precursor cell P12. Cells *a*–*e* exemplify five anatomical classes of neurones present in the mature animal; *S*₁, *S*₂ are somatic cells; X indicates cell death.

The rest of the nematode, although long ignored, has turned out to be by far the most rewarding to study. The nerve ring sends out a ventral cord which runs the entire length of the animal and contains about 60 motor neurones. Muscles in the ventral half of the body send processes to the ventral cord. The dorsal cord, which services the muscles in the dorsal half, is discontinuous, being made of pieces sent up from the ventral cord³⁹.

A closer look reveals that this swimming motor system consists of 12 almost identical wiring groups in tandem, each with five classes of cell. Developmental studies^{40,41} indicate that these groups arise from 12 precursor cells which divide several times each (Fig. 1). The pattern of division and specialization is intriguing. After a division, the posterior daughter cell is almost always more like the parent than the anterior cell, and is also more likely to be programmed to die. Relative position may be the crucial cue for nematode differentiation. Although the different groups retain different classes of cells, the same pattern of division is always followed (Fig. 1). For example, although in all female *C. elegans* cell *c* dies, it is always necessary to make cell *c* so that its sister can go on to divide into cells *a* and *b*. More remarkably, in two of the groups, cell *b* is also necessarily made and killed so as to produce a cell *a* (Fig. 1).

Again, the wiring diagram leads immediately to speculation about how the circuit works. Cells *a* and *b* look like antagonists, one (who knows which) excitatory, the other inhibitory. Cell *d* might be responsible for the deep ventral bend which seems

necessary to reverse direction. Cell *e* is perhaps part of a reciprocal inhibition system used to coordinate movement. If these guesses are correct, then *C. elegans* swimming, like the flight of many insects, is myogenic, confirming an earlier and equally elegant behavioural analysis⁴².

C. elegans, quite unexpectedly, has been found to be a very useful animal for developmental studies. It is possible in principle to establish the lineages of all of its cells, and this has already been done for 180 of its 550 embryonic cells⁴³. Post-embryonically, new neurones are formed (Fig. 1) and some old ones undergo remodelling⁴⁴. In larvae, class *d* cells innervate only the ventral muscles, whereas in adults they innervate only dorsal muscles. A study with mutants⁴⁴ sheds some light on the general rules of neural development in *C. elegans*. One mutant lacks all the late-developing motor neurones. These are the only cells which innervate the class *d* nerves. Nevertheless, the *d* cells rewire themselves as usual even in the absence both of normal input from the neurones which drive it and of others which take over its connections to the ventral muscles. In *C. elegans*, at least, no synaptic input is necessary to make, break and remake normal synaptic output connections. Unfortunately, the reverse is true of other animals, especially mammals. In any case, despite a limited behavioural repertoire and the impracticability of physiological confirmation, nematodes are providing fascinating information about neural development.

At this stage the same questions might be answered by laser-beam lesions rather than mutations, but the ultimate power of mutant analysis is clear. For example, a mutation has recently been found which rearranges the pedigree of developing nerve cord cells, and another interferes with the cell divisions shown in Fig. 1, producing multinucleate neurones with strikingly novel morphology (H. R. Horvitz, personal communication).

Songs and sensory circuits of insects

Most of the *Drosophila* nervous system is quite complex, and neural analysis of behaviour patterns is slowly progressing from the outside of the animal in. For example, courting males sing a species-specific 'love song' by extending and vibrating their wings towards females⁴⁵. The circuits underlying this behaviour can be dissected with gynandromorphs—animals which are part male and part female. Flies with male heads and female bodies will extend their wings to females as if in courtship, so apparently the 'will' to begin the song is located in the brain^{46–48}. However, the fly goes on to produce a proper *Drosophila* song only if there is some male tissue in the thorax as well; the actual pattern-generating circuit is probably located in the thoracic ganglion. This parallels the situation in crickets, where males with specific brain lesions are normally incapable of song production, although a headless cricket body will produce a complete song if the thoracic ganglion is stimulated⁴⁹. In both species, the decision to sing and the actual song pattern are mediated by separate neural structures. Other experiments with hybrid crickets⁵⁰ suggest that the male circuit which generates the courtship song pattern is specified by the same genes as the female circuit which recognises the song (they may even be the same circuit).

Crickets have also been used to study neural development. One single-gene mutation causes the loss of the mechanoreceptive filiform hairs on the cricket cerci⁵¹. As a result, the mutant shows no sensitivity to touch on the cerci. The sensory nerves from the missing hairs are completely normal, but the large interneurone to which they project is 'withered.' Apparently, normal stimulation is unnecessary for the maintenance of receptor cells, but is crucial for the integrator cell. This has striking parallels in mammalian visual development. In cats, for example, failure to see out of one eye during a critical period leaves the receptor unaffected, but prevents the normal development of the visual cortex, causing irreversible blindness in one eye⁵². We would like to believe that there is a general principle of neural organisation here somewhere, and the dis-

covery that cricket cerci have a critical period makes that possibility more likely⁵³. If one cercus is waxed over for a time early in development so that the hairs cannot be stimulated, the animal becomes deaf on that side. Removal of the wax has no effect.

Mouse mutants

Mouse brains, although small and unintelligent, are organised like ours and thus allow investigation by conventional neuroanatomical, histological and physiological techniques. Moreover, large areas of the mammalian brain, such as the cerebellum and visual striate cortex, are composed of similar circuit elements repeated over the surface of the brain^{54,55}; one can therefore study a small patch of neural tissue with the expectation that the wiring in the rest of the structure follows the same rules. Finally, there is the hope that functional insights gained with any mammalian brains may be applied to ourselves, and that genetically derived pathologies in mice may have clinical analogues in man. However, the system has one great disadvantage: it is currently too expensive deliberately to induce mutations in mice and screen the progeny for a desired phenotype. This means relying on inbred stocks which have been retained because they acted very abnormally—reeling, trembling, twirling, staggering, and so on. Most neurological mouse mutants chiefly affect the cerebellum, whose anatomy is conveniently simple and repetitive, with only five types of cell, and whose circuitry and synaptic interactions are well understood⁵⁴. Unfortunately, the mutants isolated so far are drastic^{56,57}. Two, *weaver* and *staggerer*, cause massive degeneration of granule cells (the most numerous neurones, which receive the major input from the rest of the brain). Two more, *nervous* and *pcd*, lead to loss of the Purkinje cells (the largest neurones, which provide the only output from the cerebellum).

Genetic lesions such as these are useless for understanding the subtleties of neural development or finding out how the normal cerebellum functions to control posture, coordination and balance. They do, however, raise interesting questions about neurogenesis. For example, the mutation *pcd*, in addition to rapid degeneration of cerebellar Purkinje cells, also produces slow degeneration of mitral cells in the olfactory bulb and photoreceptors and induces structural abnormalities in spermatozoa, leaving other cells in the animal unaffected^{57,58}. Are the four affected cell types under similar metabolic stress, which is in some way aggravated by the mutation? It is difficult to see what they have in common. Do the neurones occupy analogous positions in an unrecognised developmental hierarchy, and thus become dependent on the same controlling element? Evidence from other mutations would be needed to consider such an idea seriously.

One important question is whether the primary lesion is actually in the affected cells. Unlike those of nematodes, vertebrate neurones may die from unrequited synapses, either because they lack proper input or because they fail to establish connections with their target cells. Thus, normal granule cells could degenerate because cryptically abnormal Purkinje cells refuse to interact with them or vice versa. This issue has been elegantly settled for the *pcd* mutation. Mullen⁵⁸ made chimaeric mice⁵⁹ whose cerebella contained mixtures of *pcd* and normal cells. By properly selecting parent strains and using enzyme markers, he was able to ascertain the genotypes of individual cells and it was thus possible to show that all the *pcd* cells had atrophied and all the normal cells survived. Therefore, the defect is cell autonomous: that is, if a Purkinje cell dies it is its own (genetic) fault. This is unsurprising. However, the opposite is true for *reeler*, a mutation which generally affects neuronal migration during development and specifically induces abnormal, twisted morphology in Purkinje cells. In chimaeric normal *reeler* cerebella, Purkinje cells of both genotypes were sometimes normal-looking, sometimes abnormal⁶⁰. In this case the morphology of the cells is specified from outside, by the neural environment.

With the mutant *staggerer*, there is some indication that the major observed effect, granule cell degeneration, is due to a defect in the Purkinje cells which fail to form proper dendritic spines, the normal target for granule cell synapses^{61,62}.

So far, in spite of these elegant analyses, the mouse mutants have taught us little about the development of the cerebellum and nothing about its function. However one is of particular interest; *reeler* affects not only the cerebellum but cerebral cortex as well, altering its development radically. The embryonic cortex forms and develops as a hollow shell of cells around the fluid-filled lumen of the anterior neural tube. Neuronal cell divisions occur only on the inside surface of the shell. In normal development, the earliest neurones to form tend to remain near the lumen to form the deepest layer of the cortex, and cells formed at later stages migrate past all the others to lie in more superficial layers. The position of a neurone in the cortex is thus determined largely by the time of its last cell division, with the youngest cells on the outside. In *reeler*, the developing cortical neurones fail to migrate past each other so that the oldest cells remain in superficial layers with younger cells progressively deeper⁶³.

Caviness⁶⁴ has examined the visual area of this 'inside-out' cortex and found that incoming fibres carrying sensory information from the eyes terminate in the correct layer; that is, instead of penetrating the cortex from below, making a few synapses in the deepest layer, then rising to terminate at an intermediate layer, as in the normal mouse, the same fibres immediately ascend to the most superficial cortical layers, form some synapses, then reverse direction to terminate in intermediate layers. This inversion rule also holds true for different classes of output axons coming from the cortex.

When he examined individual neurones in *reeler* cortex, Caviness found that some looked normal, others were apparently inverted, and still others had axons which abruptly reversed direction after leaving the cell soma. The findings above, taken *in toto*, look very discouraging from the mouse's point of view. Nevertheless, physiological recordings by Dräger⁶⁵ show that most of the cells in the *reeler* striate cortex have normal retinotopic organisation and visual response

characteristics, including binocularity and orientation and direction selectivity. The upside-down *reeler* cortex is functionally indistinguishable from the rightside-up normal one.

No one has studied chimaeric cortices containing both normal and *reeler* cells. It would be interesting to see whether normal organisation is enforced on the mutant neurones and, if not, whether proper visual receptive fields persist with competition between normal and inverted cortical organisation.

It is worth noting that Siamese cats are also neurological mutants. The gene (temperature-dependent albinism) which specifies the pale colour of their body fur also reduces the pigmentation of the cells behind the retinas in their eyes⁶⁶. This pigment deficit apparently induces dramatic miswiring of optic nerve fibres carrying visual information to the brain, causing many to reach the wrong destination. This miswiring leads to a cascade of neurological defects in centres involved with vision—in the thalamus, in primary and secondary visual cortex, and perhaps in even higher areas. Although the original mutant defect is probably non-neural, work with the visual system of these cats (reviewed in refs 67–69) may tell us much about the developmental rules which normally lead to proper arrays of neural connections and orderly maps in the mammalian brain.

Conclusion

At its inception 15 years ago, the neurogenetic approach seemed very promising. Since then it has led to many elegant experiments, much tantalising information, and some insight. In general, however, behavioural geneticists have asked one set of questions and answered another. The central questions of neurobiology—the chemical bases of ion gating and synaptic transmission, the mechanisms involved in specifying a neurone's shape and determining its interaction with other cells, the changes in neurones and circuits which underlie learning—are all untouched by the approach. The field is still promising, in the best and the worst sense. If progress in the next decade equals that of the last, the promise will begin to be fulfilled.

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articles

Very large array observations of solar active regions

Kenneth R. Lang & Robert F. Willson

Department of Physics, Tufts University, Medford, Massachusetts 02155

The first very large array (VLA) synthesis maps of solar active regions are presented here. The observational capabilities, and the basic techniques of using the VLA to observe the Sun are also described. Comparisons of VLA maps with magnetograms are also made.

OBSERVATIONS of the Sun with three-element interferometers at centimetre wavelengths and angular resolutions of a few arc seconds have shown that solar active regions contain intense (10^5 – 10^6 K), small-scale (less than $15''$), circularly polarised (above 30%) sources which dominate solar emission at these wavelengths and angular resolutions^{1,2}. The radiation from these sources has been interpreted in terms of gyromagnetic processes involving strong magnetic fields and thermal electrons in the dense coronal regions above sunspots^{2–6}. As long as the active regions are not emitting flares, the intensity, angular size, and degree of circular polarisation of the small-scale sources remain remarkably constant for intervals of at least 12h. This stability makes these solar sources ideal candidates for the aperture synthesis technique which has been developed by

radioastronomers to obtain two-dimensional maps of celestial radio sources with arc second angular resolution⁷. When applied to the centimetre wavelength radiation of the Sun, these techniques may be used to test the gyro-resonance absorption theory of this component of solar emission. Moreover, the synthesis technique can be used to obtain high resolution maps of circular polarisation which delineate the magnetic field structure in the solar corona. These structures can then be compared with optical wavelength Zeeman effect observations of the magnetic field in the underlying photosphere and chromosphere. Because of scintillations in the terrestrial atmosphere, these optical wavelength observations are limited to the angular resolutions which are now available at radio wavelengths using the synthesis technique. The first synthesis maps of solar active regions at centimetre wavelengths were obtained using the 12 25-m paraboloids (20 interferometer pairs) of the Westerbork synthesis radio telescope (WSRT)^{8,9}. In this case, a synthesised beam of $6'' \times 22''$ at a wavelength of 6 cm was used to show that solar active regions contain small-scale sources (with peak brightness temperatures as large as 10^6 K and circular polarisations of $\sim 50\%$) surrounded by a lower intensity $\sim 10^5$ K halo.

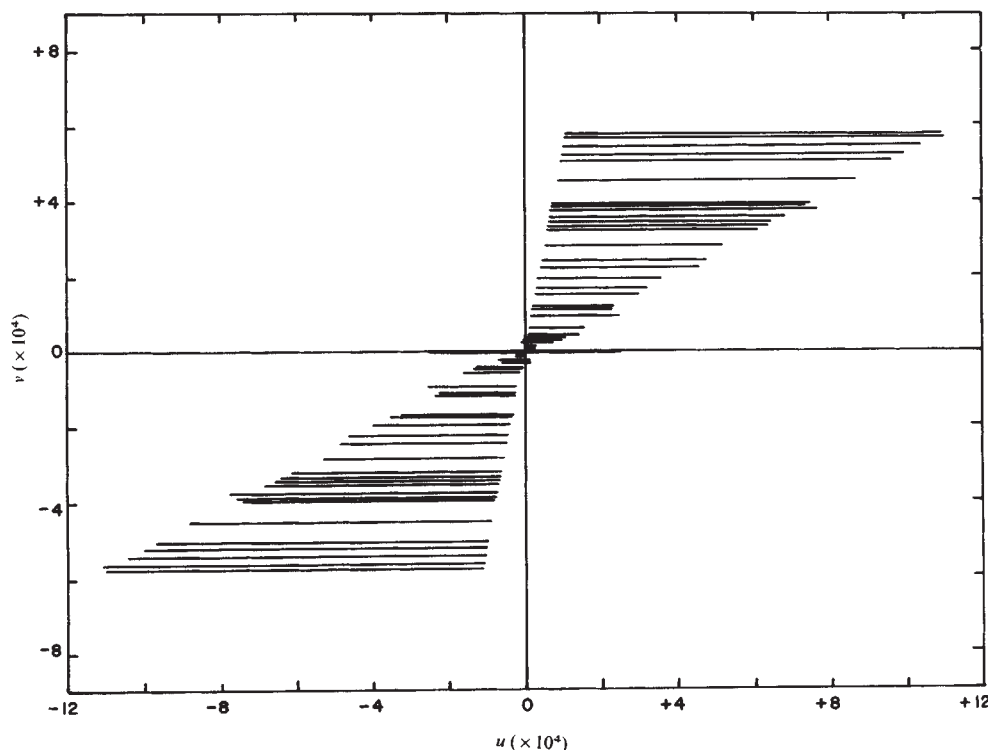


Fig. 1 The u - v tracks obtained while observing the Sun at 6 cm wavelength with 45 interferometer pairs of the VLA when the Sun is at a declination of 0° . Because the longer baselines were not used in our maps, we have omitted here the 10 tracks from the 10 interferometer pairs connecting to the furthestmost antenna located 10.473 km from the array centre. The dark line at $v = 0$ denotes the tracks obtained while observing the Sun at 6 cm wavelength with the 20 interferometer pairs of the WSRT when the Sun is at a declination of 0° .

Although circularly polarised emission was observed from both sunspots and plage peaks, the more intense emission came from sunspots whose magnetic polarity coincided with the direction of circular polarisation. We describe here synthesis maps of solar active regions obtained using 12 25-m paraboloids (65 interferometer pairs) of the VLA now under construction near Socorro, New Mexico by the National Radio Astronomy Observatory¹⁰. In this case a synthesised beam of $1'' \times 3''$ is obtained at a wavelength of 6 cm.

Construction and operation of the VLA

Twenty-seven antennas are being constructed, tested and placed along the three arms of the WYE configuration of the VLA. The position angles of the north, south-east, and south-west arms are, respectively, 355° , 115° and 236° ; whereas the geodetic coordinates of the array centre are $34^\circ 04' 43.497''$ N (latitude) and $107^\circ 37' 03.819''$ W (longitude). With this configuration and location the most complete spatial frequency coverage is obtained when the Sun is at its larger positive declinations. The

permanent antenna stations are placed along each arm at distances ranging between 0.045 and 21.0 km from the array centre, thereby providing effective angular resolutions ranging between $0.57''$ and $4.44''$ at a signal wavelength of 6 cm for the interferometer pairs along each arm (each combination of two antennae makes one interferometer pair). Similar angular resolutions are available for the interferometer pairs consisting of antennae placed on different arms. The individual antennae have 25 m diameter main reflectors with a feed system¹¹ designed to receive signal wavelengths of 1.3, 2, 6 or 21 cm with respective beamwidths of 1.9, 2.9, 8.6 and $30'$ and respective aperture efficiencies of 46, 54, 65 and 50%. About 1 min is required to rotate the antenna feeds to sample signals at different wavelengths. Two independent systems simultaneously accept two oppositely polarised signals at the chosen wavelength, and these signals are converted into intermediate frequency (IF) signals with bandwidths of 1.5 MHz, 12 MHz or 50 MHz. The two IF signals received at each antenna are sent to a central location where they are multiplied together to give the correlated flux for each oppositely polarised signal for every interferometer pair. Each correlated signal is time averaged to give its amplitude and phase every 10 s.

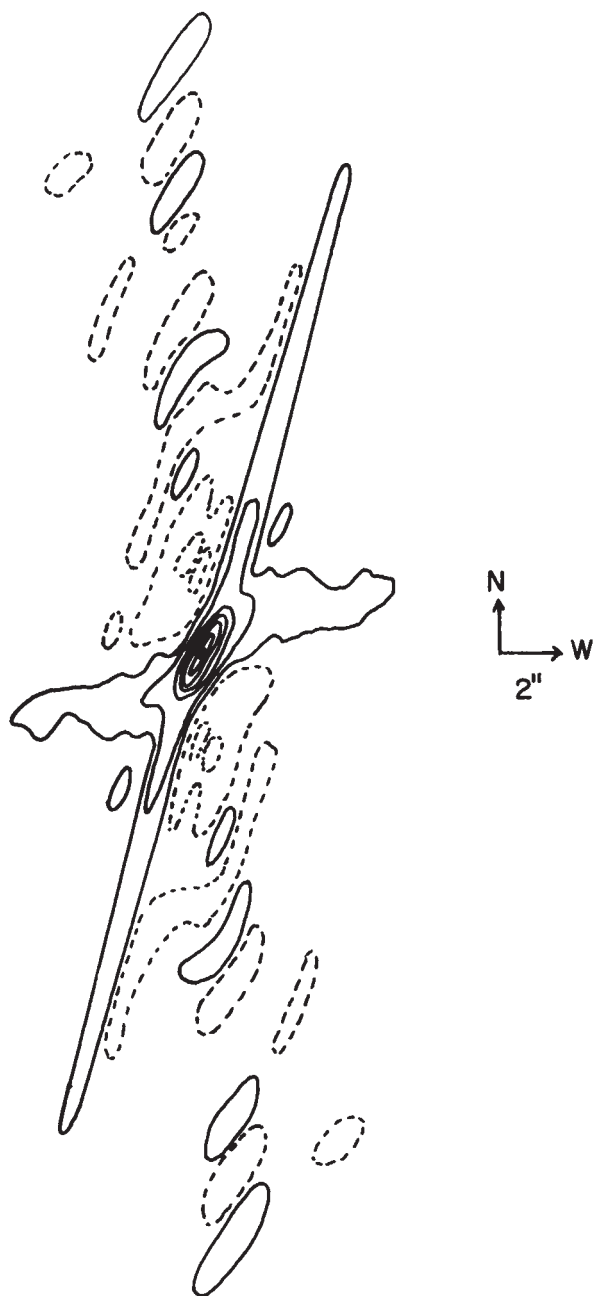


Fig. 2 The dirty VLA beam pattern at a signal wavelength of 6 cm constructed from observations during the daylight hours on 1 April 1978.

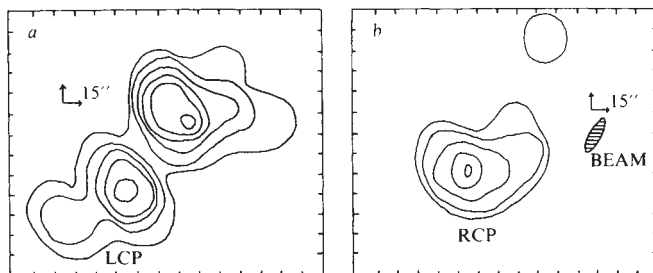


Fig. 3 VLA synthesis maps of solar active region 1046 obtained with one days observation on 30 March 1978. North is up, west is to the right. *a*, The left; and *b*, the right hand circularly polarised signals; $\lambda = 6$ cm.

The present digital system of the VLA is designed to handle up to 12 antennae with two IF signals each, but more sophisticated electronics designed to handle 27 antennae with four IF channels each are being installed. For the observations reported here we used 12 antennae with three antennae positioned along the south-east arm at 0.080, 0.090 and 0.147 km from the array centre and nine antennae placed along the south-west arm at 0.045, 0.484, 0.710, 0.970, 1.589, 3.188, 5.223, 7.659 and 10.473 km from the array centre. Altogether the average correlated flux from 55 interferometer pairs was sampled every 30 s at a signal wavelength of 6 cm for both the left hand circularly polarised (LCP) and right hand circularly polarised (RCP) signals. When observing the Sun at centimetre wavelengths the system noise is completely dominated by the solar brightness temperature of 10^4 – 10^6 K. The theoretical r.m.s. noise fluctuation in the correlated flux density obtained with each antenna pair is 6.4 Jy ($1 \text{ Jy} = 10^{-23} \text{ erg s}^{-1} \text{ cm}^{-2} \text{ Hz}^{-1} = 10^{-26} \text{ W m}^{-2} \text{ Hz}^{-1}$) for a system noise temperature of 10^4 K with our chosen bandwidth of 12 MHz and an integration time of 30 s. We later averaged the data over 5 min intervals to obtain a r.m.s. noise of 2 Jy for each interferometer pair.

Active regions 1046 ($24^\circ\text{S } 7^\circ\text{W}$ at 12h UT on 30 March) and 1056 ($12^\circ\text{N } 65^\circ\text{E}$ at 0h UT on 31 March) were observed on 30 March and 1 April 1978 respectively, at a signal wavelength of 6 cm. For each antenna pair the correlator outputs were calibrated by observing 3C 84 or CTA102 for 5 min every 20 min, whereas the solar active region was observed during the remaining 15 min of each 20-min period. The frequent observations of the calibrator lessened the effects of tropospheric refraction variations, and calibrated the instrumental gain, polarisation and phase fluctuations.

Measurements

For each polarisation of each antenna pair the correlated flux, $S_{mn}(\text{Sun})$, and the corrected phase $(\phi_m^* - \phi_m) - (\phi_n^* - \phi_n)$ when observing the Sun were taken to be the amplitude and phase of the visibility function, $V(u, v)$. Because corrections for the motion of the Sun across the sky, its rotation about its axis, and its horizontal parallax were incorporated in the computer program which determined the pointing of each antenna, the changing values of the visibility function could then be used to measure the brightness distribution of the active region $I(x, y)$, through the fundamental Fourier transform relationship

$$V(u, v) = I(x, y) \exp [+ 2\pi i(ux + vy)] \quad (1)$$

where the visibility function and the brightness distribution are determined separately for the two circular polarisations, u and v are the projections of the baseline vector in the direction of increasing right ascension and declination, respectively; and x and y are, respectively, coordinates in the direction parallel to increasing right ascension and declination. These quantities, defined in radians, are given by

$$\begin{aligned} u &= B_x \sin(h) + B_y \cos(h) \\ v &= -B_x \sin(\delta) \cos(h) \\ &\quad + B_y \sin(\delta) \sin(h) + B_z \cos(\delta) \end{aligned} \quad (2)$$

where B_x , B_y and B_z are the topocentric coordinates of the baseline vector in units of the observation wavelength, and h and δ respectively denote the hour angle and declination of the active region.

As the Earth rotates, the hour angle of the active region changes and the projected baseline describes an ellipse in the u - v plane (the Fourier plane of the brightness distribution). The extent of the coverage in the u - v plane depends on the Sun's declination, the orientation and length of the baseline vector of each interferometer pair, and the total number of interferometer pairs. During the observations reported here, for example, the Sun was near the vernal equinox when the Sun's declination passes through zero degrees, and the elliptical u - v tracks degenerate into straight lines. Figure 1 shows the u - v tracks when the Sun is observed at zero degrees declination with the VLA and with interferometers which are orientated in a strictly east-west direction such as the Westerbork synthesis radio telescope (WSRT). For the east-west baselines, v becomes zero for a declination of 0° . Later in the summer or winter, when the Sun's declination can respectively reach $+23^\circ$ and -23° , the extent of the v coverage by the WSRT broadens. For the VLA, the straight lines given in Fig. 1 become shortened or lengthened ellipses, respectively, for the southern and northern declinations.

The correlated flux when observing the Sun for each polarisation with each antenna pair (m, n) was determined from the equation

$$\begin{aligned} S_{mn}(\text{Sun}) &= \frac{R_{mn}(\text{Sun}) [T_{sm}(\text{Sun}) T_{sn}(\text{Sun})]^{1/2}}{R_{mn}(\text{cal}) [T_{sm}(\text{cal}) T_{sn}(\text{cal})]^{1/2}} S_{mn}(\text{cal}) \\ &\quad \times \exp [i(\phi_m^* - \phi_m) - i(\phi_n^* - \phi_n)] \end{aligned} \quad (3)$$

where R_{mn} denotes the correlation coefficient (fraction of the total power which is correlated) for one polarisation of the antenna pair (m, n).

$$\begin{aligned} R_{mn}(\text{cal}) &= G_m G_n \frac{S_{mn}(\text{cal})}{[T_{sm}(\text{cal}) T_{sn}(\text{cal})]^{1/2}} \\ &\quad \times \exp [i(\phi_m - \phi_n)] \end{aligned} \quad (4)$$

where G_m and ϕ_m respectively denote the magnitude and phase of the complex gain of the detection system of antenna, m , the $S_{mn}(\text{cal})$ is the correlated flux when observing the calibration source with one polarisation of the antenna pair (m, n), and $T_{sm}(\text{cal})$ denotes the system noise temperature when observing the calibrator source with one polarisation of antenna, m .

When observing the solar active region the correlation coefficient, $R_{mn}(\text{Sun})$, for one polarisation of the antenna pair (m, n) is given by

$$\begin{aligned} R_{mn}(\text{Sun}) &= G_m^* G_n^* \frac{S_{mn}(\text{Sun})}{[T_{sm}(\text{Sun}) T_{sn}(\text{Sun})]^{1/2}} \\ &\quad \times \exp [i(\phi_m^* - \phi_n^*)] \end{aligned} \quad (5)$$

Because the parametric amplifiers were used for both solar and calibration observations, an extra IF attenuation producing a phase difference $(\phi_m^* - \phi_m)$ had to be inserted when observing the Sun. This phase difference was measured by observing the calibration source with each antenna with the attenuator in and out. The levels of the two IF signals being fed into each multiplier from each antenna were kept at a fixed value using an automatic level control loop in which the sampled signal level controls an attenuator in the final IF amplifier. The automatic level control loop therefore automatically compensates for the variation in gain magnitude caused by the introduction of an extra IF attenuator when observing the Sun. This meant that we could assume that the gain magnitudes are the same when observing the calibrator and the Sun ($G_m = G_m^*$). The system noise temperature, $T_{sm}(\text{cal})$, when observing the calibrator was measured for each antenna using a switched noise source whose temperature of $\sim 3\text{K}$ was accurately calibrated for each antenna by observing the Moon. The system noise temperature, $T_{sm}(\text{Sun})$, when observing the active region was measured for each antenna by determining the difference in the total power detected when observing the active region and a quiet Sun region whose temperature was assumed to be 10^4K . Even at these high temperatures the front-end amplifiers were in their linear, unsaturated regions. Subsequent observations of more intense active regions with antenna temperatures $\geq 10^5\text{K}$ did, however, require the removal of the parametric amplifiers to avoid saturation. In this case, the gain and phase could be calibrated by observing 3C273 with the amplifiers out, but it also had to be observed with the amplifiers replaced in order to measure the system noise temperatures, $T_{sm}(\text{cal})$.

The correlated flux of the calibration source, $S_{mn}(\text{cal})$, was taken to be 49 and 34 Jy, respectively, when observing 3C84 and CTA102. The gain of each circularly polarised channel was calibrated by adjusting the digitised output of the correlators to give the same values of $R_{mn}(\text{cal})$ for every baseline pair and both polarisations. This is essentially because the calibrator sources have negligible circular polarisation and they are effectively point sources for the interferometric baselines used. These instrumental adjustments were also applied to the solar data to obtain the corrected amplitude, $S_{mn}(\text{Sun})$, and the corrected phase $(\phi_m^* - \phi_m) - (\phi_n^* - \phi_n)$ for each polarisation of each antenna pair every 30 s of each 15-min period of solar obser-

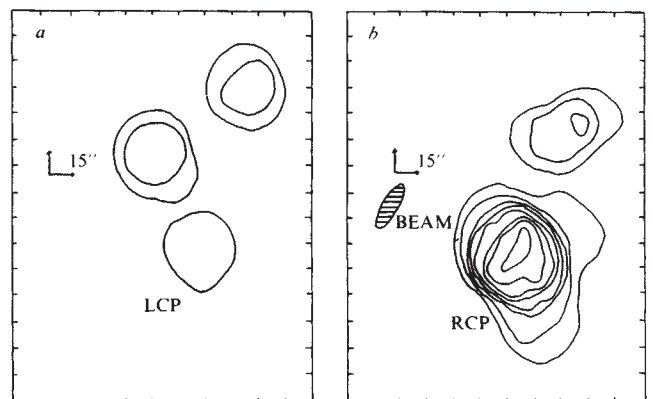


Fig. 4 VLA synthesis maps of solar active region 1056 obtained with one days observation on 1 April 1978. North is up, west is to the right. *a*, The left, and *b*, the right hand circularly polarised signals; $\lambda = 6\text{ cm}$.

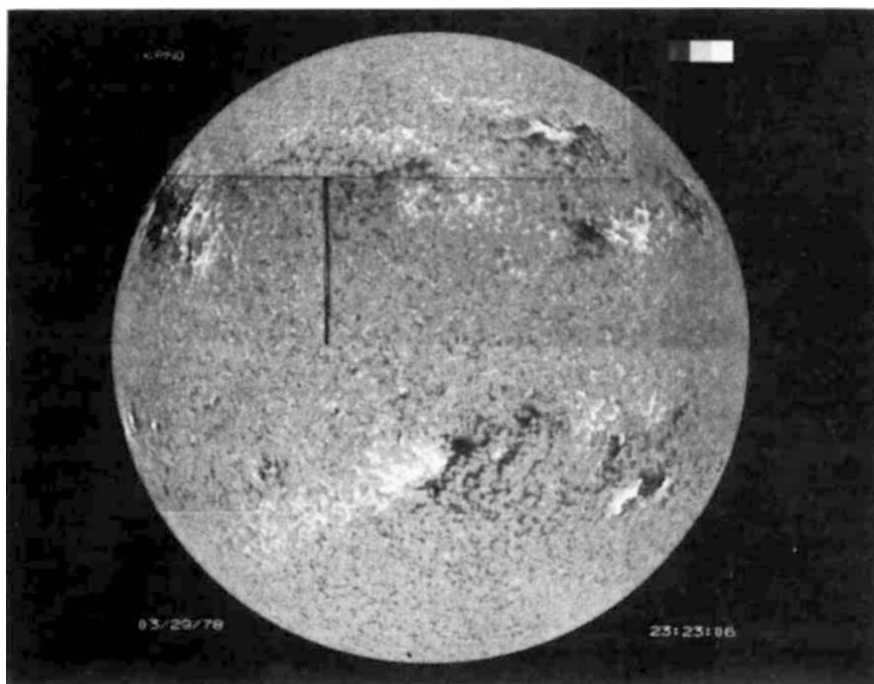


Fig. 5 A Kitt Peak National Observatory magnetogram of the Sun taken on 29 March 1978. Here Zeeman effect observations of the 8,680 Å line of neutral iron have been used to derive magnetic field structure which is limited by scintillation to 2"–3" angular resolution. As in Figs 3 and 4, north is up and west is to the right, and the dark and light regions respectively refer to regions of negative and positive polarity. The angular scale for the magnetogram is determined by the 32' angular diameter of the Sun. The general features of AR 1046 (lower centre) and AR 1056 (upper left) compare well with the VLA maps shown in Figs 3 and 4.

vations. The 30 s values of amplitude and phase were then added together to obtain 5-min vector averages with a phase calibration better than 5° and an amplitude calibration accurate to <10%. These calibrated data were taken to be the amplitude and phase of the source visibility function, and the source intensity distribution was obtained by Fourier transforming the calibrated data using the Tufts University DEC-10 computer.

VLA synthesis maps and comparisons with magnetograms

The procedure we have implemented involves a direct synthesis of a 'dirty' map which is the convolution of the true brightness distribution with the response function of a point source. This response function, or dirty beam is shown in Fig. 2. It has a FWHM of 1"×3", and the highest sidelobe is 14% of the maximum response.

A deconvolution of the dirty map was then performed using the clean procedure in which the effects of the 14% sidelobes of the synthesised beam were reduced¹². Because both active regions observed were resolved on baselines having fringe spacings <15" we have deleted those data having fringe spacings less than this value; thereby minimising the map noise and reducing computer time. The resultant total intensity contour maps are shown in Figs 3 and 4, where the synthesised beam is represented by a shaded ellipse. Here the contour levels represent 0.9, 0.8, 0.7, 0.6, 0.5, ... 0.2 times the maximum response which had the values of 102, 80, 17 and 5 Jy per synthesised beam area (in units of arc s²), respectively, for AR1046 (LCP), AR1046 (RCP), AR1056 (RCP) and AR1056 (LCP). The r.m.s. sensitivity of the VLA system for 12 h of observation of a 10⁴ K Sun is estimated to be 0.06 Jy.

The most striking aspect of the maps shown in Figs 3 and 4 is that the small-scale sources shown in the LCP and RCP maps are not spatially coincident. They suggest the feet of small dipole magnetic fields with positive magnetic polarity corresponding to the regions with strong right circular polarisation. Region 1046, for example, contains two sources ≈30"×30" in size which are ≥80% LCP with one source of similar angular size and ≥60% RCP located between them. Region 1056 contains one intense component about 30"×30" in angular size with ≥70% circular polarisation. In all cases the small-scale sources are surrounded by less intense halo emission ≥1' in angular extent (large angular sizes >1' are undersampled by the VLA system). Assuming a source size of 30"×30" an intensity of 100 Jy per synthesised beam area (7"×20") and a beam efficiency of 80%, we obtain a

source brightness of ~10⁻¹¹ erg s⁻¹ cm⁻² Hz⁻¹ rad⁻². Using the Rayleigh-Jeans law with a wavelength of 6 cm and this value of brightness we obtain a brightness temperature ≈10⁶ K. Thus the small-scale sources and the magnetic structures which give rise to their circular polarisation belong to the higher, hotter, coronal regions of the solar atmosphere. Assuming that the radio emission is at the first few harmonics of the gyrofrequency, magnetic field strengths of the order of a few hundred Gauss are inferred².

The magnetic structures obtained with the VLA can be compared with full disk magnetograms taken daily at the Kitt Peak National Observatory (2"–3" angular resolution) and the Mount Wilson Observatory (12" angular resolution). The magnetograms refer to the cooler regions in the medium to high photosphere, whereas the VLA maps refer to the hotter, higher levels of the solar corona. Figure 5 shows a Kitt Peak magnetogram taken on 29 March 1978 (because of inclement weather both Kitt Peak and Mount Wilson could not obtain data on our days of VLA observation on 30 March and 1 April 1978). The general features for the magnetogram of AR 1046 (in the lower centre of Fig. 5) compare well with the magnetic structure seen in the VLA maps (Fig. 3). The dark feature of negative polarity centred about an axis inclined ~45° West of North corresponds to a similar region in the LCP map, whereas the RCP feature corresponds to the more intense white region of positive polarity. The width of ~1', the extent of ~4', and the separation of components (30"–1') are in agreement for the two maps. Similarly, the magnetogram of AR 1056 (in the upper left hand corner of Fig. 5) agrees well with the VLA map (Fig. 4), for both indicate a predominantly positively polarised region (RCP) of a few arc min in extent. Thus we may conclude that the general magnetic structure inferred from magnetograms for the photosphere agrees with that obtained by VLA observations for the coronal regions.

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Optimal wavelength region for communication with extraterrestrial intelligence: $\lambda = 1.5$ mm

N. S. Kardashev

Space Research Institute, Moscow, USSR

This article shows that the best capability and reliability of communication at astronomical distances can be achieved at millimetre wavelengths. The cost of communication, and the number of directions in which signals are sent and searched for are assumed to be fixed. A characteristic frequency might be 203.385 GHz, corresponding to the splitting of the ground state of the lightest atom—positronium—and coinciding with the centroid of the relic background spectrum.

SINCE the pioneer work of Cocconi and Morrison¹ it has been assumed that, the optimal search range for transmissions from extraterrestrial civilisations is at short decimetre wavelengths² (near the 21-cm line of atomic hydrogen, the most abundant element). This spectral band lies near the intensity minimum of the cosmic radio background, and the 21-cm line itself has a symbolic value that narrows down the range of frequencies to be searched (it is supposed that transmitting frequency is near the line).

The choice of the decimetre wavelengths remains valid even if there is no information available about the locations of transmitting and receiving parties. But some papers^{3,4} point to the advantage of millimetre wavelengths for other strategies of space communication and take into account the scattering of radio waves in interstellar plasma. The aim here is to provide some confirmation of it.

Let us consider the opposite case: the transmitting side knows a fixed number of directions where signals are to be transmitted. This number might be defined, for example, by the number of stars with conditions suitable for the appearance of life and civilisation. The same assumption can be made by the receiving side: they search for signals from a fixed number of the nearest stars of the required type or from the direction towards the galactic centre. This strategy permits the channelling of signals in narrow (and hence very economical) beams. The assumption about the mutual directivity of transmitting and receiving antennae has been used in analysing most of the previous searches for CETI signals (CETI, communication with extraterrestrial intelligence)

Maximum information capacity and reliability

Let us determine the maximum information flow (or its maximum redundancy if the flow is fixed), with a limited amount of expenditure on the construction and the maintenance of the communication system. What wavelength will be optimal in this case? Assume that P_m and $\Delta\nu$ are the mean power and the bandwidth of the transmitter; A_t and A_r are the effective areas of the transmitting and receiving antennae; T is the noise temperature of the receiving system; R is the distance between the receiver and the transmitter; n_t and n_r are the number of directions in which signals are sent and searched for, respec-

tively. The mean spectral density of the received signal is

$$S_\nu = (P_m A_t A_r) / (R^2 \lambda^2 n_t n_r)$$

The spectral information capacity of a communication channel taking into account quantum fluctuations according to Lebedev and Levitin⁵ is:

$$C_\nu = \ln[1 + z_\nu(1 - e^{-x})] + [z_\nu + (e^x - 1)^{-1}] \times \ln\{1 + [z_\nu + (e^x - 1)^{-1}]\} - x(e^x - 1)^{-1} \quad (1)$$

where $z_\nu = S_\nu/h\nu$ is the spectral density of the number of signal photons and $x = h\nu/kT$ (here ν is the signal frequency h and k are the Planck and Boltzmann constants). Equation (1) does not contain limitations related to the physical nature of the transmitter and the receiver.

For $z_\nu \ll 1 - e^{-x}$ and $z_\nu \ll (e^x - 1)^{-1}$, we have $C_\nu \approx xz$, and the total information capacity is:

$$C = \Delta\nu C_\nu = (P_m A_t A_r) / (kTR^2 \lambda^2 n_t n_r) = (S_\nu \Delta\omega) / (kT) \quad (2)$$

in agreement with the well-known Shannon formula. It is important to note that for small signals, equation (2) holds true for any frequency and the quantum effects play no role⁵. In this respect, quantum fluctuations have often been treated inadequately in papers on the CETI problem.

Assume that the expenditures of each side, including maintenance and construction of the system, are $\Sigma \propto A^\alpha/\lambda$, $\alpha = 1 \div 2$, that is, the cost increases somewhat faster than the antenna area and is inversely proportional to the wavelength, λ . We believe that this dependence is not anthropocentric because it is associated with expenditures proportional to the mass of the structure and the manufacturing accuracy, taking into account real physical effects.

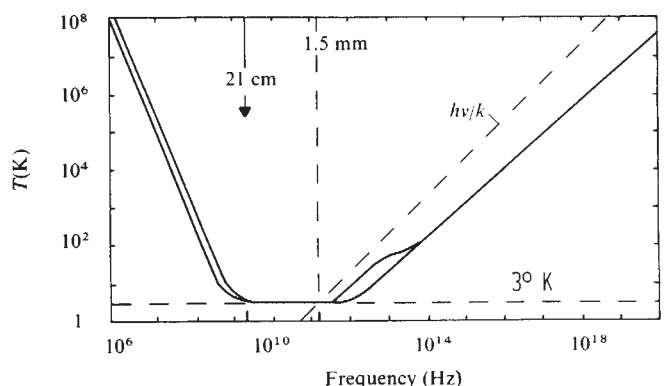


Fig. 1 Frequency dependence of brightness temperature of the cosmic background radiation (solid line). The data in radio, sub-millimetre and infrared ranges are given for the galactic pole (lower curve) and for the galactic plane (upper curve). The vertical broken line is the centroid of the relic background spectrum.

Let us also assume that the expenditures on the transmitter do not depend on the wavelength (the power of laser generators is the same as, if not higher than, radio generators), and are determined only by the amount of energy consumption.

The value T in the limiting case is determined by the brightness temperature of the cosmic background radiation. Figure 1 shows the frequency dependence of the brightness temperature determined by setting the observed sky intensity to the Planck function at each frequency^{2,6,7}. The dashed line $T = h\nu/k$ ($x = 1$) divides observational data in two parts corresponding to classical and quantum fluctuations.

These data show that:

$$T = \begin{cases} \propto \lambda^{+2.6} & \text{for metre and decimetre waves} \\ = 3 \text{ K} & \text{for centimetre and millimetre waves} \\ \propto \lambda^{-1} & \text{for submillimetre and shorter waves} \end{cases} \quad (3)$$

With the assumptions discussed above, we obtain:

$$C \propto \begin{cases} \lambda^{-(1.6 \text{ to } 2.6)} & \text{for metre and decimetre waves} \\ \lambda^{-(0 \text{ to } 1)} & \text{for centimetre and millimetre waves} \\ \lambda^{+(0 \text{ to } 1)} & \text{for submillimetre and shorter waves} \end{cases} \quad (4)$$

The millimetre range therefore seems to be most efficient. Some other arguments in favour of this range should be added.

Let us consider some empirical data about the effects of interstellar plasma on the communication system^{8,9}. The effect of pulse broadening due to the dispersive properties of the plasma is $\Delta t_0 = 8.3 \times 10^{15} \times DM \times \Delta\nu \times \nu^{-3}$ s, where DM is the dispersion measure ($\text{cm}^{-3} \text{ pc}$), $\Delta\nu$ and ν are in Hz. Such a distortion of pulses can be partially eliminated by signal processing, depending on how accurately the dispersion measure is known. The dispersive effect is negligible when

$$\Delta\nu < \Delta\nu_0 = 1/\Delta t_0 = 1.1 \times 10^{-8} \times \nu^{3/2} (DM)^{-1/2} \quad (5)$$

Interstellar scintillation is significant for

$$\nu < \nu_c = 8 \times 10^8 (DM)^{0.75} \text{ Hz} \quad (6)$$

In the case of scintillation, the average time when there is no signal within a bandwidth is:

$$\delta t_s = 2.5 \times 10^{-5} \nu (DM)^{-1} \text{ s}$$

where the bandwidth is smaller than

$$\delta\nu_s = \begin{cases} 10^{-26} \nu^4 (DM)^{-2} \text{ Hz, for } DM < 10 \text{ cm}^{-3} \text{ pc} \\ 10^{-24} \nu^4 (DM)^{-4} \text{ Hz, for } DM > 10 \text{ cm}^{-3} \text{ pc} \end{cases} \quad (7)$$

Scattering also results in a longer duration of signal pulses, given by:

$$\Delta t_s = 0.1/(\delta\nu_s) \quad (8)$$

as well as spectral broadening of quasi-monochromatic signals:

$$\Delta\nu_s = 1/(\delta t_s) \quad (9)$$

Note that the value $\Delta\nu_s$, although small, exceeds the effect of the band broadening associated with the uncertainty or the variability of the Doppler shift of a signal frequency $\Delta\nu_D$. The latter effect also limits the bandwidth to $\Delta\nu \geq \Delta\nu_D = A\nu^{1/2}$ (ref. 10). However, future methods of frequency control and generation should provide the accuracy $\Delta\nu_D \approx 10^{-20} \nu t^{-1/2}$ where t is the measurement time in seconds¹¹. The widest band will then be obtained with $t = 1/\Delta\nu_D$, and will be equal to $\Delta\nu_D \approx 10^{-40} \nu^2$, that is, much less than $\Delta\nu_s$. $\Delta\nu_D$ is also negligibly small for frequencies higher than the critical ν_c (the band $\leq \Delta\nu_D$ can hardly be used because of the small information content and difficulties in generation, detection and receiving).

All the effects mentioned here set limits to possible signal bands and pulse durations. Figure 2 gives regions of possible bands for optimal signals ($\Delta\nu = 1/\Delta t$). Pulses from the transmitter will not fade as long as $\Delta\nu > \delta\nu_s$ or $\Delta\nu < \Delta\nu_s$. There is no signal distortion if $\Delta\nu < 10\delta\nu_s$ and $\Delta\nu < \Delta\nu_0$. In the latter case, the distortion can be partly corrected by signal processing.

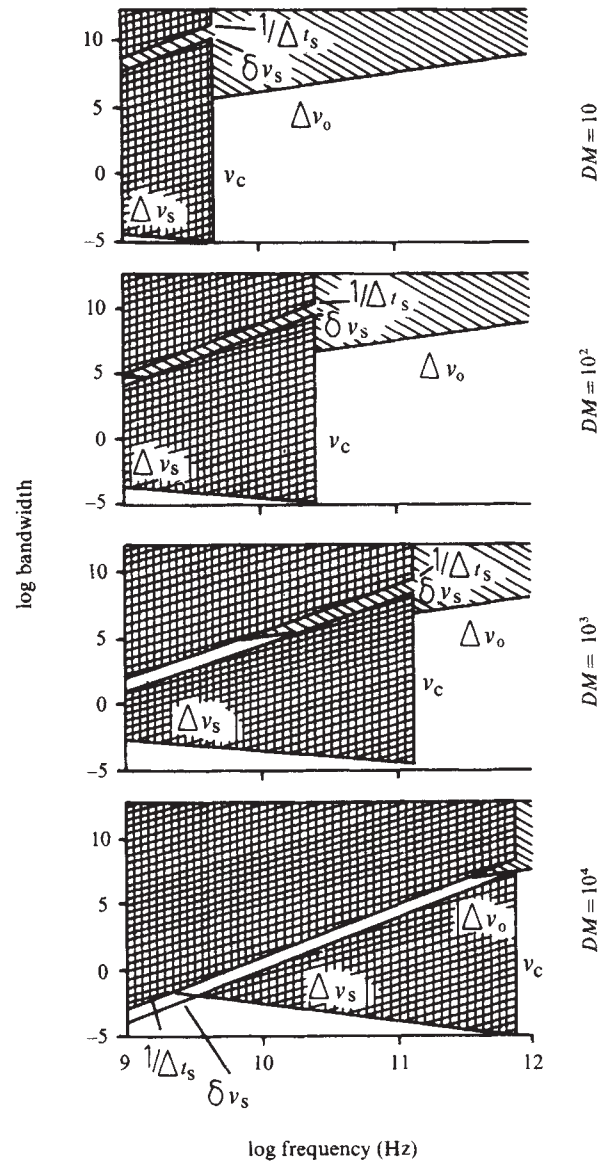


Fig. 2 Limits on the bandwidth and pulse duration ($\Delta\nu = 1/\Delta t$) due to the effects of interstellar plasma. Open regions: no limits. Hatched regions: partial restoration of pulse is possible by processing. Doubly hatched regions: attenuated signals cannot be restored.

Note that all of the plasma effects mentioned above decrease rapidly at higher frequencies indicating the advantage of the millimetre range, especially at large distances. The best region corresponds to $\nu > \nu_c$, and since $DM < 10^3 \text{ cm}^{-3} \text{ pc}$ for most of the Galaxy, the millimetre range satisfies this requirement.

Selection of characteristic frequency

We now discuss several frequencies with outstanding symbolic value in the millimetre range. First of all, this band contains most of the energy of the cosmological relic background radiation, the intensity of which dominates all other radiation in this range. The 'centroid' of the relic background spectrum may be considered as a basic characteristic frequency. Integrating the Planck radiation law as follows:

$$\int_0^{x_r} x^3 (e^x - 1)^{-1} dx = \int_{x_r}^{\infty} x^3 (e^x - 1)^{-1} dx$$

one obtains $x_r = 3.50$, which corresponds to a frequency centroid $\nu_r = 197$ to 220 GHz, or a wavelength $\lambda_r = 1.36$ to 1.52 mm for a relic background temperature from 2.7 to 3.0 K (ref. 12).

By a remarkable coincidence a unique radio spectral line falls in this band. It corresponds to the splitting of the ground state of the lightest atom—positronium (P_s). Positronium forms by recombination of an electron and a positron and is about 1,000 times lighter than a hydrogen atom. Laboratory investigation of positronium provided some of the basic evidence for the validity of quantum electrodynamics. The characteristic feature of positronium is the fact that its properties can be calculated from fundamental physical constants. The lifetime of this atom is very short because of the annihilation process, so a high concentration could hardly be expected in natural cosmic conditions.

The ground state of a positronium atom ($n = 1$, $l = 0$) is split into singlet 1S_0 and triplet 3S_1 states (where the positron and electron spins are antiparallel and parallel, respectively). According to most accurate theoretical estimates, this splitting corresponds to a frequency of 203,383 267(86) GHz ($\lambda = 1.47$ mm).

The calculated frequency was confirmed with high accuracy in the experiment by Egan *et al.*¹³ to be 203,384 9(12) GHz.

The annihilation times for the given sublevels are: $\tau(^1S_0) = h/(\pi m_e c^2 \alpha^5) = 1.25 \times 10^{-10}$ s for the two γ quanta annihilation, and $\tau(^3S_1) = 9h/[4(\pi^2 - 9)m_e c^2 \alpha^6] = 1.4 \times 10^{-7}$ s for the 3γ -quanta annihilation (m_e is the electron mass, c is the velocity of light, α is the fine structure constant).

These values may be used as adequate symbols to determine, for instance, the pulse duration of a beacon, and that of the basic information flow. Their reciprocal values (8 GHz and 7 MHz respectively) could denote signal bandwidths. We can estimate the maximum recurrence period for such pulses. If only one pulse is emitted in each direction and if such pulses are sent in a sequence to 10^{11} stars, then the signal recurrence period will be 12.5 s and 3.9 h, respectively.

We shall now estimate the information content of a millimetre channel on the basis of current technology. Given $P_m = 1$ MW, $A_t = A_r = 40$ m² (corresponding to a reflector 10 m in diameter), $T = 200$ K, $\lambda = 1.5$ mm, $R = 100$ pc, $n_t = 10^3$, $n_r = 1$, then from equation (2), C is 2 nit per day [where 1 nit = $1/\ln 2 = 1.44$ bit. If $P_m = 100$ MW and $A_t = 4,000$ m² (100-m reflector) are assumed for the transmitting side, the same result is obtained for $R = 10$ kpc.

Interestingly, monochromatic radiation near the frequency 203 GHz has already been detected from astronomical objects. Phillips *et al.*¹⁴ observed the radio line from the $3_{13}-2_{20}$ transition of the $H_2^{18}O$ molecule, the laboratory frequency of which (203,407 5 GHz) is strikingly close to the positronium-line frequency. Observations were carried out with an antenna of diameter 11 m and a system temperature of 200 K. The line was discovered in emission from molecular clouds near Ori A and

DR 21. The line intensity corresponds roughly to the expected concentration of $H_2^{18}O$ molecules in these clouds. It should be stressed that with such equipment, it is already possible to search for CETI sources in the millimetre range.

In conclusion, we emphasise that the millimetre region is outstanding among all possible ranges of electromagnetic radiation because it is:

(1) A spectral region of minimum brightness temperature (for lower frequencies, the temperature increases due to synchrotron radiation, for higher frequencies, due to radiation of dust and stars).

(2) A spectral region of minimum absorption and scattering. The most economical strategy of space communications is to align the transmitting and receiving antennae and to send the radiation and the information it carries in a narrow beam.

In this case, the features discussed above for the millimetre range in the present cosmological epoch provide for maximum information capacity and reliability of communication for a given expenditure on construction and organisation.

The most prominent millimetre wavelength, within the bandwidth corresponding to the centroid of the relic background is that of the positronium line at 1.47 mm, which nearly coincides with the $H_2^{18}O$ line. Thus a search for CETI signals would yield simultaneous astronomical results. Alternatively, during normal astronomical observations of the $H_2^{18}O$ line there is a possibility of chance discovery of artificial signals. We once again emphasise the necessity of searching for monochromatic and pulsed signals, bearing in mind that beacons might have a fairly large duty cycle. The use of such signals appreciably reduces the average power required for the transmitter. This fact, along with the known frequency and bandwidth, greatly facilitates the search for extraterrestrial intelligence.

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A novel bacteriophage defence mechanism: the anti-restriction protein

Nikolaus Spoerel & Peter Herrlich

Max-Planck-Institut für Molekulare Genetik, Berlin-Dahlem, FRG, and Institut für Genetik der Universität Karlsruhe and Kernforschungszentrum Karlsruhe, Institut für Genetik und für Toxikologie von Spaltstoffen, Postfach 3640, D 7500 Karlsruhe 1, FRG

Thomas A. Bickle

Biozentrum der Universität Basel, Klingelbergstr. 70, CH-4056 Basel, Switzerland

Bacteriophage T3 and T7 protect their DNA from restriction by producing, as the earliest detectable phage functions, anti-restriction proteins. Although the two phage proteins differ in their chromatographic and antigenic properties, they act by the same mechanism: the anti-restriction proteins inhibit E. coli K12 restriction endonuclease by direct interaction.

A PARTICULARLY vulnerable stage of the bacteriophage life cycle is immediately after infection, when the naked DNA is exposed to host cell nucleases, particularly restriction endonucleases. Some bacterial viruses have developed means of combating the host-controlled restriction system by modifying their DNA by, for example, incorporating unusual bases or by glycosylation¹. The modified DNA is protected from degradation *in vivo* and *in vitro*. The related *Escherichia coli* phages T3 and T7 apparently possess a different mechanism to overcome

Table 1 Effect of increasing Ado-met concentration on the *EcoK* inhibition by SAMase

-SAMase Initial Ado-met (mM)	+SAMase		ATPase activity (% of control)	Final Ado-met (mM)
	ATP hydrolysed (% of input)	ATP hydrolysed (% of input)		
0.5	46.5	6.0	13	0.05
1	61.7	22.7	37	0.1
2	57.7	28.2	49	0.2
5	51.9	31.2	60	1.6
10	41.0	21.0	51	5.6

EcoK and SAMase were purified as described elsewhere¹⁰⁻¹⁴ (SAMase A (refs 10, 11) was used in all experiments; SAMase B (refs 10, 11), however, behaved identically). ATPase was assayed using ³H-ATP (refs 12-14) and resolving ATP and ADP by thin-layer chromatography on polyethyleneimine after the incubation. Initial Ado-met concentration was calculated assuming '100% purity' of the dry substance used (Boehringer). ¹⁴C-Ado-met levels were determined after chromatography on cellulose thin layer developed with 64% ethanol²¹. Incubations were for 30 min at 30 °C. The incubation volume of 20 µl contained the components and amount of SAMase described in the legend to Fig. 1 except that the concentration of Ado-met was varied. All components were present from the start of incubation.

restriction: they grow on K12 and B strains even though their DNA is a substrate for the restriction enzymes *in vitro*². Mutants of these phages can be found which are efficiently restricted by the *E. coli* systems. Such mutations map in the first gene of T3 (*S*-adenosyl-methionine hydrolase (SAMase), ref. 3 and N.S. *et al.*, unpublished) and also in the first gene of T7 ('0.3' gene', 'M'-gene⁵). Because the T3 mutations occur in a gene coding for an enzyme that hydrolyses *S*-adenosyl methionine (Ado-met), one of the essential cofactors for the *E. coli* K12 and B (*EcoK* and *EcoB*) restriction enzymes, one could explain the anti-restriction activity as a direct consequence of the hydrolysis of Ado-met. No such explanation can be offered for anti-restriction by T7 because this phage does not code for a SAMase.

T3 SAMase and the T3 anti-restriction activity are independent functions of the same protein

Most T3 mutants which are defective in anti-restriction have lost SAMase in the same mutational event (ref. 3 and N.S. *et al.*, unpublished). One *ts* mutant, however, could not overcome restriction at the non-permissive temperature, although it could still induce SAMase activity (ref. 6 and N.S. *et al.*, unpublished; see also Table 4). The inverse phenomenon has also been described: a mutant (3356) retained the anti-restriction phenotype but had lost the SAMase activity⁷. This can be explained by assuming either that the same peptide chain carried two independent activities or that the two activities were exerted by two proteins encoded in the same DNA segment but read in different reading frames⁸. The latter possibility was ruled out, as out of 20 mutants isolated that were defective in both SAMase and anti-restriction activities, seven were found to be *amber* mutants. The isolation of an *amber* mutant has also been reported from another laboratory³. All these *amber* mutants expressed both anti-restriction and SAMase activities in suppressing hosts. As the *amber* mutation is reading-frame specific⁹, both activities must be expressed by the same polypeptide. Thus, the genetic evidence suggests that SAMase activity is not required for anti-restriction.

SAMase inhibits the restriction system *in vitro*

The effect of purified SAMase^{10,11} on restriction was tested using the *EcoK* system. The restriction reaction proceeds in a series of steps¹²⁻¹⁴, including the activation of *EcoK* by Ado-met, the binding to DNA, the triggering of *EcoK* by the unmodified

DNA site to become an endonuclease, and the ATP-dependent endonucleolytic cleavage. The last reaction involves ATP hydrolysis, and ATPase activity was, therefore, taken as a measure of the overall reaction. This activity was indeed strongly inhibited by SAMase (Tables 1, 2). To elucidate which step in the chain of reactions leading to ATPase activity was inhibited, the steps were examined individually. The possibility that SAMase inhibits by blocking access to the *EcoK* recognition sites on the DNA was unlikely, as we demonstrated by gel filtration chromatography that SAMase did not bind to DNA (not shown). The binding of *EcoK* to the DNA depends strictly on the presence of Ado-met¹²⁻¹⁴. Degradation of Ado-met *in vitro* will, of course, block this initial step and all subsequent steps. Various lines of evidence have, however, shown that Ado-met hydrolysis is not the cause or not the only cause of anti-restriction.

Anti-restriction does not depend on Ado-met hydrolysis and SAMase interacts directly with *EcoK*

If the anti-restriction depended on Ado-met hydrolysis, one would expect to be able to relieve the inhibition simply by increasing the initial concentration of Ado-met. In the experiment shown in Table 1, incubations were made with a constant amount of both *EcoK* and SAMase but different concentrations of Ado-met. The amount of ATP hydrolysed by *EcoK* and the amount of Ado-met remaining were measured. Although the Ado-met concentration varied over a 20-fold range, and its concentration at the end of the experiment varied between close to zero and more than five times that necessary to support a maximum *EcoK* reaction, the amount of inhibition by SAMase was constant at about 50% except at the lowest Ado-met concentration. At this low Ado-met concentration the SAMase

Table 2 Interaction between *EcoK* and SAMase

Complete mixture* preincubated for 10 min, then addition of:	ATP hydrolysed (% of input)
—	17.0
150 ng SAMase	0.2
75 ng SAMase	4.0
30 ng SAMase	4.2
15 ng SAMase	11.8

* The incubation volume of 20 µl contained the components described in Fig. 1. All components except SAMase were present from the start of incubation. ATP hydrolysis between 10 and 30 min is shown.

hydrolysed essentially all the Ado-met before the *EcoK* reaction could begin. Above the minimum concentration of Ado-met, the inhibition of *EcoK* apparently required no Ado-met hydrolysis. This result indicates that the inhibition is due to a direct interaction between SAMase and *EcoK*.

Further support for this idea came from a series of experiments with the Ado-met analogue, *S*-adenosyl homocysteine (SAH), which is a potent inhibitor of both *EcoK* and SAMase^{10,11,15}. SAH competes with Ado-met in the first step of the *EcoK* reaction. If SAH was present from the beginning of the incubation, *EcoK* ATPase activity was inhibited (Fig. 1a). On addition of SAMase, *EcoK* was inhibited further, even with high SAH concentrations (Fig. 1a), despite the fact that Ado-met hydrolysis was prevented (Fig. 1c). As expected from the reaction mechanism, SAH was unable to inhibit *EcoK* if it was added late in the reaction at times when free Ado-met was no longer required (Fig. 1b). However, when SAMase was added with SAH late in the reaction, inhibition was observed. The degree of inhibition was constant over all SAH concentrations and thus independent of the amount of Ado-met hydrolysis (Fig. 1b). In fact, it depended solely on the amount of SAMase (Table

2). Changing the proportion of SAMase to *EcoK* caused inhibition of ATPase of between 0 and 100%. The time course (not shown) and reversibility (see below) of the reaction suggest a non-catalytic interaction. Although the proportion of active enzyme in both preparations *EcoK* and SAMase is somewhat uncertain, an approximately stoichiometric interaction is compatible with our data.

The experiment in Fig. 1b showed that SAMase could inhibit *EcoK* after proper binding to DNA. The inhibition of activated *EcoK*-DNA complexes was also measured directly. Such complexes, purified by passage through Biogel, hydrolyse ATP. The hydrolysis was still inhibited by the addition of SAMase (not shown) although Ado-met was no longer required for *EcoK* ATPase and was only present in concentrations (10^{-10} M) far below the K_m of SAMase^{10,11}. These data confirm the conclusions from the genetic results described initially that Ado-met hydrolysis is not required for anti-restriction and that the SAMase protein purified for its affinity to SAH^{10,11} causes anti-restriction by interaction with *EcoK* after the Ado-met-mediated step.

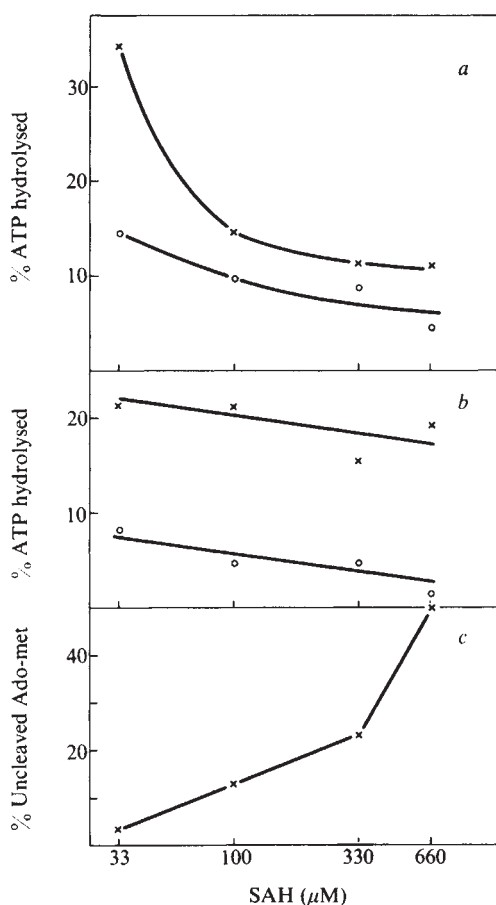


Fig. 1 The influence of SAH on anti-restriction *in vitro*. The reaction mixtures contained in a total volume of 20 μ l (refs 12-14): 2 μ mol HEPES, pH 8.0, 1.4 nmol EDTA, 60 nmol 2-mercaptoethanol, 0.12 μ mol $MgCl_2$, 0.2 μ g unmodified λ (0) DNA, 1.1 nmol purified Ado-met¹²⁻¹⁴ (^{14}C Ado-met at 22 Ci mol^{-1}), 2 nmol ATP (3H -ATP at 540 Ci mol^{-1}), 100 ng *EcoK* plus 60 ng SAMase where indicated. SAH was added as plotted in the abscissa. ATPase activity was determined as described elsewhere¹²⁻¹⁴. a, Action of SAMase on the overall reaction. All components were present at 0 min. Incubation was for 30 min. $\times$, Minus SAMase; $\circ$, plus SAMase. b, Action of SAMase on the activated DNA-Ado-met-*EcoK* complex. ATP, SAH and SAMase were added after 10 min of preincubation of the other components. Further incubation was for 20 min. $\times$, Minus SAMase; $\circ$, plus SAMase. c, Ado-met hydrolysis in the incubation mixtures of a. Ado-met remaining at the end of incubation was plotted (in %).

Table 3 Inhibition of SAMase anti-restriction by antibodies

	ATP hydrolysed (% of input)	ATPase activity (%)
Expt 1		
-SAMase (control)	32.9	100
+SAMase at 10 min	8.5	26
+SAMase plus anti-SAMase at 10 min	22.4	68
Expt 2		
-SAMase (control)	40.3	100
+SAMase	12.1	30
+SAMase, + additional Ado-met at 10 min	15.9	39
+SAMase, + anti-SAMase at 5 min	10.4	26
+SAMase, + anti-SAMase at 5 min, + additional Ado-met at 10 min	29.3	73

The assays and incubation mixtures were as in Table 1 and Fig. 1. The anti-SAMase antibodies were prepared as described^{10,11} (anti-SAMase A and anti-SAMase B were equally effective). The amount of antibody added did not interfere by itself with *EcoK* ATPase. SAMase was present from the start of incubation where indicated.

Experiments with antibodies against SAMase directly support the idea of an interaction between the two enzymes, *EcoK* and SAMase. Not only could anti-SAMase added with SAMase to the preincubated *EcoK*-DNA complex totally prevent inhibition (Table 2, expt 1), it could also reactivate *EcoK* after SAMase had been permitted to act (Table 3, expt 2). This clearly shows that the interaction between SAMase and *EcoK* is readily reversible because, if it were not, the antibodies would have precipitated *EcoK* with the SAMase. The reversibility of the interaction was confirmed in gel filtration experiments (not shown): the association of the two enzymes did not survive passage through a Biogel column.

The antibody experiment also provided support for the model of the *EcoK* reaction mechanism¹²⁻¹⁴, which predicts that cleavage of Ado-met after the initial step does not influence the subsequent course of the restriction reaction. The antibodies are somewhat unusual because although they precipitate SAMase, they do not inhibit its enzymatic activity^{10,11}. In the experiments shown in Table 3, both ATP hydrolysis and Ado-met cleavage (not shown) were monitored. As expected, the presence of the antibodies did not prevent the hydrolysis of essentially all the Ado-met, but had a dramatic effect on the inhibition of the activated *EcoK*. Restriction could proceed in the absence of Ado-met.

Properties of purified mutant SAMase enzymes

To investigate whether the anti-restriction reaction observed *in vitro* reflected the physiological anti-restriction mechanism, we isolated mutant proteins of phage with known *in vivo* phenotypes and tested their behaviour *in vitro*. The mutant proteins were purified as cross-reacting material from lysates of cells infected with different SAMase mutant strains, using anti-SAMase immobilised on agarose. The properties of all except one of the isolated mutant proteins were what would be expected from their phenotype *in vivo* (Table 4): the point mutant SAMase 17 eliminated both anti-restriction and SAMase *in vivo*; the isolated protein showed neither activity *in vitro*. T3 3356 could overcome restriction *in vivo* without inducing Ado-met cleaving activity—cross-reacting material from 3356 was SAMase negative but inhibited *EcoK* ATPase. So far, *in vivo* and *in vitro* phenotype correlate well, indicating the physiological role of the anti-restriction reaction *in vitro*. Cross-reacting material was also prepared from a mutant thermolabile for anti-restriction and thermoresistant for SAMase activity (SAMase ts 21). The isolated protein displayed no temperature sensitivity for anti-restriction *in vitro* (Table 4), suggesting that this thermolability occurred only in specific intracellular conditions.

Table 4 Inhibition of *EcoK* ATPase by mutant SAMase proteins

Protein from:	Incubation temperature (°C)	Ado-met hydrolysis	ATP hydrolysed (% of input)	ATPase activity (%)
—	30	—	55.4	100
Non-infected cells	30	—	46.4	84
T3 SAMase 17	30	—	38.8	70
T3 SAMase ts 21	30	+++	16.0	29
T3 3356	30	—	20.1	36
T7	30	—	42.6	77
—	42	—	72.6	100
T3 SAMase ts 21	42	+++	24.0	33

The assays were carried out as in Table 1 and Fig. 1. Mutant protein was obtained as follows: non-infected *E. coli* B_S-1 or B_S-1 at 8 min after infection^{10,11} were disrupted. An S30 was prepared and adsorbed to anti-SAMase IgG attached to Sepharose 4B. Protein was eluted by 6 M LiCl and precipitated by (NH₄)₂SO₄. 10 µg of dialysed protein was added to incubation mixtures. Non-infected cells and T7-infected cells contained no specific cross-reacting band (as judged from SDS gel). IgG was purified from anti-SAMase A antiserum^{10,11} by DEAE-cellulose chromatography²² followed by chromatography on protein A Sepharose²³ and coupled to Sepharose 4B (ref. 22). All infections resulted in <1% surviving cells. T3 SAMase 17 and 21 were isolated for inability to overcome B restriction (ref. 6 and N.S. *et al.*, unpublished). T3 SAMase 17 is a point mutation which induces no SAMase activity. T3 SAMase ts 21 induces SAMase activity *in vivo* at any temperature. T3 3356 (ref. 7) is SAMase negative, anti-restriction positive.

The T7 anti-restriction protein is unrelated to SAMase but exerts a similar action mechanism

We attempted to purify the anti-restriction protein coded by phage T7 using the anti-T3 SAMase affinity column. However, the T7 protein did not bind to the column (Table 4), indicating that it is immunologically not closely related to the T3 SAMase. Extracts from T7-infected cells also contained no cross-reacting material as judged by immunodiffusion^{10,11} and gel overlay techniques (not shown). The T7 anti-restriction protein was, therefore, partially purified using the *EcoK* inhibition assay. Although SAMase did not adsorb to DEAE cellulose^{10,11}, *EcoK*-inhibiting activity from T7-infected cells was only eluted by high salt concentrations (Fig. 2). Such activity was absent when cell extracts from non-infected cells (not shown) or from cells infected with a T7 *M* deletion mutant (Fig. 2) were used. The T7 anti-restriction protein had no affinity to SAH, which is our standard affinity ligand for the purification of SAMase. Although so different, the inactivation of *EcoK* seemed to occur by a similar mechanism, that is, direct interaction. Like SAMase, the T7 anti-restriction protein could inhibit *EcoK* ATPase at a step after the binding to DNA (Fig. 2).

Is anti-restriction the only function of SAMase and of the T7 anti-restriction protein?

Bacteriophage T3 and T7 protect their DNA from restriction by directing the synthesis of anti-restriction proteins. These proteins are the first detectable phage gene products to be made after infection. A consideration of the numerology of these proteins makes it unlikely that either Ado-met hydrolysis (in T3) or anti-restriction (in T3 and T7) are the exclusive roles of these proteins. Each *E. coli* K12 cell is estimated to contain about 10 molecules of *EcoK* (ref. 15); the intracellular Ado-met concentration is of the order of 1 µM and thus each cell contains about 500 molecules of this cofactor¹⁶. Also, the T3 SAMase is among the most abundant of the phage-coded proteins (compare the densities of protein bands in Fig. 1, tracks 5 and 12, in ref. 10) with each cell containing about 10,000 molecules^{10,11}. That is,

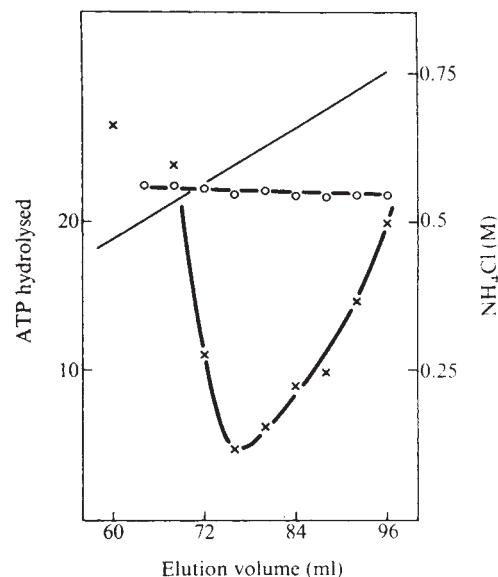


Fig. 2 Purification and action of the T7 anti-restriction protein. *E. coli* B_S-1 were infected with either T7 wild type or T7 D9 at a multiplicity of infection of 10, and collected 8 min later. The cells were disrupted as described elsewhere^{10,11} and 3 ml of an S100 adsorbed to DEAE Sephadex (10 ml bed volume) which had been equilibrated with 10 mM Tris-HCl, pH 7.5, 22 mM NH₄Cl and 1 mM dithiothreitol. Adsorbed protein was eluted using a linear gradient of 22 mM to 1 M NH₄Cl in the same buffer. Aliquots of 0.1 µl of each fraction were added to 20 µl containing activated *EcoK*-Ado-met-DNA complexes plus the components described in Fig. 1. There was no ATPase-inhibiting activity in the non-adsorbed protein. Protein eluted between 0.1 and 0.4 M salt degraded essentially all ATP, ADP and AMP. This activity was similar in both types of extracts; T7 wild type and T7 D9. The graph shows activity which was eluted beyond 0.45 M NH₄Cl. ×, T7 wild type; ○, D9. T7 D9 carries a deletion of the T7 *M* gene ('0.3 gene', see also ref. 5). Surviving cell count in both infections was 0.05%.

1,000 SAMase molecules per *EcoK* and 20 SAMase molecules per molecule of Ado-met. Similarly, the T7 anti-restriction protein is made in large amounts. We thus have the unusual situation that the enzyme is made in vast excess over its putative substrates. The overproduction suggests an additional role for both anti-restriction proteins. This has been independently suggested by experiments with pleiotropic T7 anti-restriction mutants which at the same time could not induce typical virus-dependent membrane alterations nor exclude super-infecting virus^{5,17}.

The *in vivo* time course of anti-restriction introduces another unanswered question. How does T3 and T7 DNA escape restriction before SAMase and M protein synthesis? It is well known from work with phage λ that restriction is a fast process¹⁸. T3 and T7 infection may involve a delay of restriction possibly related to the membrane alteration which occurs before protein synthesis¹⁷.

The mechanism of anti-restriction described here may serve as a model for future research into other anti-restriction systems. There are indications in the literature to suggest that anti-restriction is a general property of many bacteriophages (refs 19, 20 and S. Iida, W. Arber and T.A.B., unpublished).

During a recent EMBO workshop in Paris, we learned that K. Mark and F. W. Studier of Brookhaven National Laboratory had carried out similar experiments and obtained results in agreement with ours. We thank them for communicating their data. According to Mark and Studier (unpublished), purified T7 0.3 protein is very acidic. It inhibits the *EcoB* restriction nuclease but does not affect several type II restriction enzymes; the mechanism seemed to be stoichiometric rather than catalytic.

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letters

Detection of strong H₂O emission from galaxy NGC4945

AFTER the first detection of the $6_{16}-5_{23}$ transition of water vapour in an external galaxy by Churchwell *et al.*¹ in M33, other detections were reported by Lépine and dos Santos² in NGC253, and by Huchtmeier *et al.*³ in M33 and IC342. We report here the detection of H₂O maser emission from NGC4945, a bright edge-on spiral galaxy in the Centaurus group. This is intrinsically the most powerful H₂O maser yet detected in any astronomical source. The search for H₂O emission in NGC4945 was motivated by the previous detection of OH and H₂CO absorption by Whiteoak and Gardner⁴ and Gardner and Whiteoak⁵, and also by the similarity of this galaxy to NGC253. The observations were made on 20–22 September 1978, with the 13.7-m Itapetinga radiotelescope equipped with a ruby maser amplifier with an apparent SSB system temperature of about 150 K. The signal was analysed by a 46-channel, 100-kHz (1.35 km s⁻¹) resolution filter bank, and observations were made with the filter bank centred at different frequencies to obtain greater velocity coverage. The 4' beam was pointed towards the nucleus of the galaxy. The overall dimensions of the galaxy are 17' × 3'. The telescope was beam switched at a rate of 100 Hz. The beams were separated by 9.4 arc min and the reference beam was displaced in azimuth, which did not correspond to a fixed direction with respect to the equatorial plane of NGC4945. Antenna temperatures were corrected for atmospheric attenuation and random attenuation using the standard procedure at Itapetinga⁶ and then converted to flux units, the error in the flux scale being ~10%.

The H₂O spectrum of NGC4945 in the velocity range of 630–750 km s⁻¹ (LSR) is presented in Fig. 1. Emission features >5 Jy appear at 668, 674, 698 and 714 km s⁻¹. No other features ≥3 Jy appeared in the observed velocity range 560–770 km s⁻¹. The feature at 674 km s⁻¹ is probably not resolved and could still be stronger than 12 Jy; its intensity as observed with 100-kHz channels was confirmed by making shifts of the local oscillator. It corresponds to an intrinsic power ~10 times greater than that of the strongest H₂O source in our galaxy, W49, during its maximum period of emission, if NGC4945 is at a

distance of 4 Mpc (ref. 7). The existence of such strong H₂O sources in the Universe raises the possibility of detection of H₂O from galaxies at even larger distances; for instance, an emission similar to that of NGC4945 could have been detected with our receiving equipment if the source were at a distance twice as large.

The velocity range of the H₂O emission coincides precisely with that of the 1,612-MHz OH emission⁴, suggesting that the same region of the galaxy could be responsible for both masers.

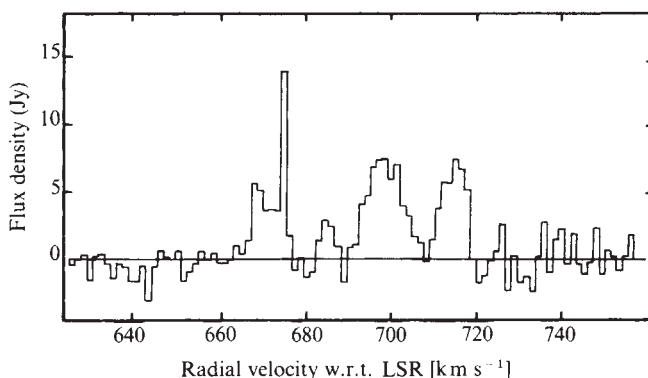


Fig. 1 The H₂O ($6_{16}-5_{23}$) spectrum of NGC4945. To convert to heliocentric velocities 4.7 km s⁻¹ should be added to LSR velocities.

Gardner and Whiteoak⁴ discuss the possibility of an association of the OH maser with the nuclear region, and point out that the far-infrared radiation from the nucleus is probably the OH pump source. It should be noted, however, that the HI profiles obtained by Whiteoak and Gardner⁸ at different positions along the optical major axis suggest that the velocity range of the H₂O emission may correspond to regions situated to the north-east of the nucleus. A more precise determination of the position of the H₂O masers with respect to the radionucleus (~30'' diameter⁹) is planned for a future run of the maser receiver.

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P. M. DOS SANTOS
J. R. D. LÉPINE

CRAAM/ON/CNPq,
Rua Ceara 290,
01243-Sao Paulo SP,
Brazil

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²³⁵U enrichment by pulse gas diffusion

LIGHT-WATER nuclear reactors use uranium fuel enriched in isotope ²³⁵U. One of the enrichment processes is gaseous diffusion. This process is used in large-scale plants in the US and uses the fact that, in the Knudsen diffusion condition, ²³⁵UF₆ diffuses through the porous membrane at slightly higher rate than ²³⁸UF₆. A transient Knudsen diffusion process is proposed here which gives a higher enrichment factor for the lighter species compared with a steady state diffusion process. Because of the higher enrichment, the number of stages required for enriching ²³⁵U to a concentration level suitable for nuclear power reactor fuel is considerably reduced.

The Knudsen diffusion coefficient is reciprocally proportional to the square root of the molecular weight. The difference in molecular weight between ²³⁸UF₆ and ²³⁵UF₆ is 0.86%, and the concentration ratio of ²³⁵UF₆ to ²³⁸UF₆ increases by diffusing through a membrane by 0.429%. Because of the low enrichment factor, many stages are required to concentrate ~0.7% ²³⁵U, in nature, to about 3% as a reactor fuel. A higher enrichment is achieved, however, if the process is operated in the transient condition described here.

Suppose a gas is imposed as a pulse (concentration C_0 for time t_0) on one side of the membrane with small pores in which diffusion is of Knudsen type (effective diffusion coefficient αD_e , membrane thickness L , and porosity a). If the gas concentration on the other side of the membrane is zero, the system is

$$a \frac{\partial C}{\partial t} = \alpha D_e \frac{\partial^2 C}{\partial x^2} \quad (1)$$

at $t = 0$, $C = 0$; at $x = 0$, $C = C_0$, $0 < t < t_0$, $C = 0$, $t > t_0$; at $x = L$, $C = 0$.

The diffusion flux Q at the outlet side ($x = L$) of the membrane is

$$Q = -\alpha D_e \left(\frac{\partial C}{\partial x} \right)_L \quad (2)$$

Substituting the solution to equation (1) into equation (2), we obtain

$$Q = \frac{C_0 D_e}{L} A \quad (3)$$

$$A = \alpha \left\{ 1 + 2 \sum_{n=1}^{\infty} (-1)^n \exp(-(n\pi)^2 \alpha T) \right\} \quad \text{for } t \leq t_0 \quad (4)$$

and

$$A = 2\alpha \sum_{n=1}^{\infty} (-1)^n \{ \exp(-(n\pi)^2 \alpha T) - \exp(-(n\pi)^2 \alpha (T - T_0)) \} \quad \text{for } t \geq t_0 \quad (5)$$

$$\text{where } T = \frac{D_e t}{aL^2} \quad \text{and} \quad T_0 = \frac{D_e t_0}{aL^2}$$

The amount of gas diffused through the membrane for the time from 0 to t_e is

$$S = \int_0^{t_e} Q dt = aLC_0 B \quad (6)$$

$$B = \alpha T_e + 2 \sum_{n=1}^{\infty} \frac{(-1)^n}{(n\pi)^2} \{ 1 - \exp(-(n\pi)^2 \alpha T_e) \} \quad \text{for } t_e \leq t_0 \quad (7)$$

$$B = \alpha T_0 - 2 \sum_{n=1}^{\infty} \frac{(-1)^n}{(n\pi)^2} \{ \exp(-(n\pi)^2 \alpha T_e) - \exp(-(n\pi)^2 \alpha (T_e - T_0)) \} \quad \text{for } t_e \geq t_0 \quad (8)$$

$$\text{where } T_e = \frac{D_e t_e}{aL^2}$$

The total amount of gas diffused through the membrane is, by putting $T_e = \infty$ in equation (8)

$$S_{\infty} = aLC_0 \alpha T_0 \quad (9)$$

The flux of the gas diffused out of the inlet side ($x = 0$) at time t (where $t > t_0$) is

$$Q' = \alpha D_e \left(\frac{\partial C}{\partial x} \right)_0 = \frac{C_0 D_e}{L} 2\alpha \sum_{n=1}^{\infty} \{ \exp(-(n\pi)^2 \alpha (T - T_0)) - \exp(-(n\pi)^2 \alpha T) \} \quad (10)$$

Let us define the effective diffusion coefficient of the heavier species ²³⁸UF₆ by D_e ; $\alpha = 1$. The effective diffusion coefficient of ²³⁵UF₆ is, therefore, αD_e with $\alpha = 1.00429$. In writing concentrations the species ²³⁵UF₆ and ²³⁸UF₆ will be distinguished, respectively, by the superscripts 5* and 8*. The concentrations in the feed are denoted by the subscript 0, and those after the diffusion through a membrane by 1.

The amount of ²³⁵UF₆ diffused through the membrane for the time from 0 to t_e is $S(\alpha = 1.00429)$ and that of ²³⁸UF₆ is $S(\alpha = 1.0)$. The concentration ratio of ²³⁵UF₆ to ²³⁸UF₆ in the collected gas is then

$$\left(\frac{C^{5*}}{C^{8*}} \right)_1 = \frac{S(\alpha = 1.00429)}{S(\alpha = 1.0)} = \frac{C_0^{5*}}{C_0^{8*}} (1 + R) \quad (11)$$

where R is the enrichment factor for the lighter species ²³⁵UF₆.

$$R = \frac{B(\alpha = 1.00429)}{B(\alpha = 1.0)} - 1 \quad (12)$$

Figure 1 shows the enrichment factor R for ²³⁵UF₆ in the gas collected for the time from $T = 0$ to T_e . When T_e is small, the amount of gas decreases, but the enrichment factor becomes very high. If the diffused-through gas is collected for the time from 0 to ∞ , the enrichment factor is $R = 0.429\%$, which is identical with that of the steady diffusion process.

Equation (10) shows that the gas diffused out of the inlet side, at $t > t_0$, also has more of the lighter component than of the heavier, but the enrichment factor for the lighter species is less than that of the steady process.

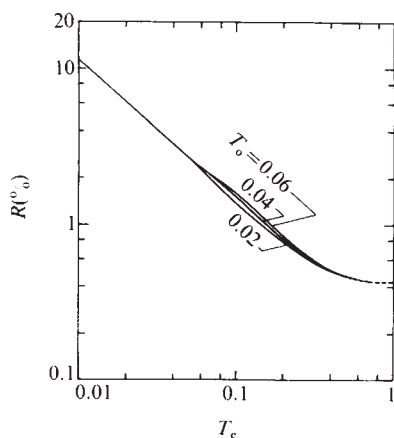


Fig. 1 Enrichment of $^{235}\text{UF}_6$ in gas collected at outlet side of membrane from time $T = 0$ to T_c .

After the pulse input is turned off, it takes some time for the gas held in the membrane to diffuse out. In case of $T_0 = 0.04$ – 0.06 , equations (5) and (10), respectively, show that no more gas diffuses out of the outlet side and inlet side of the porous membrane after $T = 0.7$. The process is then repeated by imposing the gas mixture on the inlet side of the membrane.

The enriched gas collected from the first enrichment stage is pumped to the second stage for further concentration. If N stages are used having the same operating conditions of pulse period and gas collection time, the concentration ratio of $^{235}\text{UF}_6$ to $^{238}\text{UF}_6$ in the gas obtained from the final stage is

$$\left(\frac{C^{5*}}{C^{8*}}\right)_N = \frac{C_0^{5*}}{C_0^{8*}} (1+R)^N \quad (13)$$

When 0.7% $^{235}\text{UF}_6$ is concentrated to 3%, equation (13) becomes

$$\frac{0.03}{0.97} = \frac{0.007}{0.993} (1+R)^N$$

If $R = 3\%$, the theoretical number of stages is $N = 50$. In steady diffusion process, however, the enrichment factor is only $R = 0.429\%$ and the number of stages becomes as many as $N = 345$.

Vycor type of porous glass may be used as a membrane for the enrichment process. In fact, the measurements made in our laboratory for a gaseous mixture of nitrogen and argon with a 0.12 cm-thick Vycor glass (mean pore radius $45\text{\AA}$, and porosity 30%) indicated that the lighter species, nitrogen, was considerably enriched in the gas diffused through the Vycor glass at small times, as predicted from the theory.

The effective diffusion coefficient D_e is

$$D_e = \frac{a}{\tau} D_K \quad (14)$$

where D_K is the Knudsen diffusion coefficient, and τ is the tortuosity factor. The Knudsen diffusion coefficient is

$$D_K = \frac{2\bar{v}r}{3} \quad (15)$$

where r is the pore radius. The mean molecular velocity is $\bar{v} = (8R\theta/\pi M)^{1/2}$ where R , θ and M represent, respectively, the gas constant, temperature and molecular weight. According to Satterfield and Sherwood¹, the tortuosity factor for Vycor glass is $\tau = 5.9$.

With the tortuosity value, the effective diffusion coefficient of $^{238}\text{UF}_6$ in the Vycor glass having $a = 0.3$ and $r = 45\text{\AA}$ is estimated as $D_e = 0.0002\text{ cm}^2\text{ s}^{-1}$ at 25°C .

Suppose the chambers on either side of the Vycor glass plate are, first, evacuated, and then the gaseous mixture of $^{235}\text{UF}_6$ and

$^{238}\text{UF}_6$ is introduced into one chamber as a pulse for 3 s. If the Vycor glass plate is 0.2 cm thick, $T_0 = 0.05$. If the gas diffused through the plate is collected for 0–3 s (or $T_c = 0.05$), $^{235}\text{UF}_6$ is enriched in the gas by 2.7%.

Equations (6) and (7) indicate that the amount of $^{238}\text{UF}_6$ diffused through the plate for the time from 0 to $T_c = 0.05$ is $S = aLC_0B$ with $B = 0.00028$. If the transient diffusion process is repeated in a cycle of $T_{\text{cycle}} = 0.7$, this quantity S is the amount of $^{238}\text{UF}_6$ obtained for the time $T = 0$ – 0.7 .

On the other hand, in steady diffusion condition the amount of $^{238}\text{UF}_6$ to be obtained for the same time $T = 0$ – 0.7 is $S_{\text{st}} = 0.7aLC_0$.

The amount of gas diffused through the porous membrane in the transient condition becomes much smaller than that in the steady condition. This disadvantage is unavoidable in the transient diffusion process, but it will be attractive in that the lighter species is considerably enriched in the gas obtained in a small time.

N. WAKAO

Department of Chemical Engineering,
Yokohama National University,
Yokohama, 240 Japan

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Re-evaluation of possible historical relationship between magnetic intensity and climate

HYPOTHESES concerning changes in the internal component of the geomagnetic field and consequent effects on climate^{1–10} are usually based on palaeomagnetic and palaeoclimatic data. Results are often ambiguous due to poor temporal and spatial resolution and difficulties in interpreting the proxy data^{10–12}. Use of historic data partially avoids these problems. Thus, Wollin *et al.*¹³ tentatively conclude “that the trends in intensity from most of the magnetic observatories in the world with records over at least 30 yr correlate negatively with the 10-yr means of air temperature”. Because temperature and magnetic field intensity both have regional characteristics, this conclusion implies that the relationship is a local phenomenon although it will be valid globally. This further suggests that the relationship is causal. Wollin *et al.* presented graphs of mean annual magnetic intensities from 16 magnetic observatories along with 10-yr mean temperatures from nearby meteorological stations; one example is given using a 10-yr running average of winter temperatures and five examples use annual temperatures. Although the graphical data are suggestive of the proposed relationship, the study suffers in three respects. First, data from only 22 of the 43 magnetic observatories listed are presented. Second, the use of 10-yr temperature means yields only three or four data points which may not properly represent the trend of the temperature data. Third, the degree of correlation is not quantitatively evaluated. Here we present statistical evidence that there is no globally valid inverse relationship between geomagnetic intensity and climate for the period covered by observatory records.

We initially selected 98 magnetic observatories¹⁴ which were in reasonable proximity to meteorological stations¹⁵. A record is considered usable if it contains at least 18 yr of data, a maximum number of missing data points equal to 10% of the record length, and no more than one consecutive missing datum. Missing points are linearly interpolated using data from the preceding and succeeding years. Applying these criteria leaves

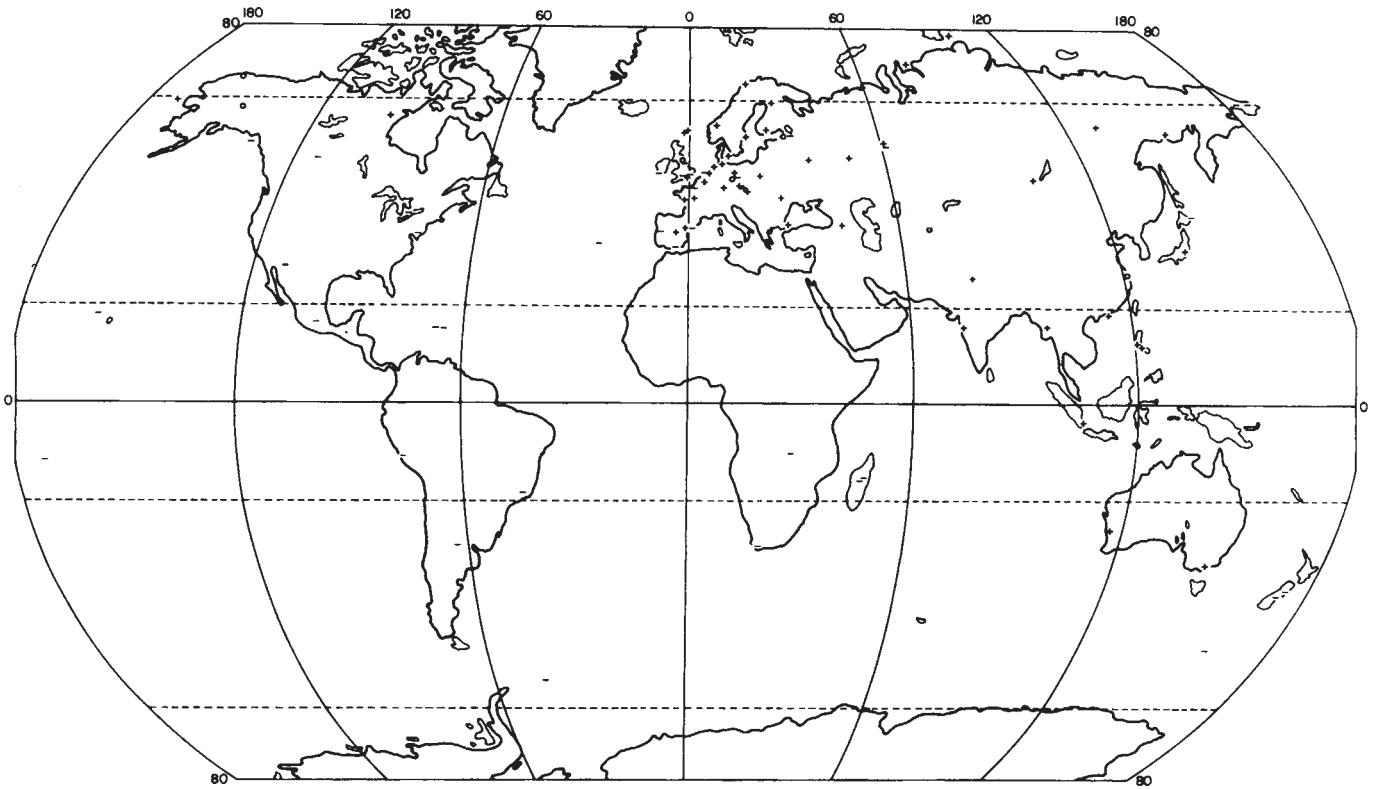


Fig. 1 Geographic pattern of change in the magnetic field strength, r_{Ft} , for 88 records (one record used twice with different meteorological stations). +, Indicates field strength linearly increasing with time (0.001 confidence level); -, indicates decrease with time; 0, indicates no significant trend. These records represent different intervals of time over the past 70 yr.

89 records from 85 stations. Only the relationship between mean annual geomagnetic intensity and temperature is investigated, even though Wollin *et al.* believe that some stations have intensities correlating with winter temperature trends. No stations are excluded from analysis *a priori*, although Wollin *et al.* suggest that the lack of correlation observed for some stations may be due to climatic influences of strong ocean currents.

We investigated the possibility of a linear relationship between temperature and intensity. Cross plots of these two variables show no evidence of a monotonic nonlinear relationship. Correlation coefficients between magnetic intensity and time (r_{Fi}), temperature and time (r_{Ti}), and temperature and intensity (r_{TF}) were computed for the various records. The geographical distribution of magnetic observatories and the trends for the r_{Fi} values are shown in Fig. 1. The histograms of Fig. 2 summarise results from all records used for the three correlation coefficients. Since the magnetic intensity variation has been linear (low frequency), we did not consider the possibility of a frequency dependent relationship. Zero lag was used in computing r_{TF} which seems reasonable considering the linear variation of intensity and our primary concern in trends rather than high frequency changes. The z transformation of Fisher¹⁶ is used to transform the correlation coefficient r by the formula $z = 0.5 \log_e(1 + r/1 - r)$. As r goes from -1 to 1 , z goes from $-\infty$ to $+\infty$. This transformation is advantageous because for any population of correlation coefficients, the distribution of z will be nearly normal, and the standard error of z will have the simple form $S_z = 1/(n-3)^{1/2}$ where n is the number of points for which r is calculated. Figure 3 shows a histogram of the transformed z/S_z values corresponding to r_{TF} . If the r_{TF} values were randomly drawn from a single distribution with a population mean of zero, the z/S_z values would have a standard normal distribution. It is obvious from the outliers that the values do not represent a single population. To quantify the results, we applied several statistical tests¹⁶⁻¹⁸ to the z_{TF} data. Assuming that the

z values are drawn from a single population, a weighted mean and standard error of the mean can be estimated as $\bar{z} = \sum z/S_z^2 / \sum 1/S_z^2$,

$$S_z = \frac{1}{(\sum(n-3))^{1/2}}$$

with the summation taken over the n records. The calculated values are $\bar{z} = -0.89$ and $S_z = 0.019$. A χ^2 test can then be used to see if the z values differ significantly among themselves, the test statistic being $\chi^2_{N-1} = \sum(n-3)(z - \bar{z})^2$. The calculated value is $\chi^2_{88} = 20,140$ which is highly significant, indicating that the z values and hence the correlation coefficients of r_{TF} do not represent a single population. Thus, the $\bar{z}$ and S_z values derived above cannot be considered valid population estimates. A χ^2 test can then be used to test for an overall association between intensity and temperature by use of the statistic $\chi^2_N = \sum(n-3)z^2$. For this test, $\chi^2_{89} = 22,373$ which is also highly significant. The χ^2 statistics are highly influenced by the relatively few outliers observed in Fig. 3. Because of the linear nature of the geomagnetic intensity temporal variation seen in the r_{Fi} histogram of Fig. 2, whenever a temperature record is long and linear, the result will generally be a high z_{TF} value. So the χ^2 tests are not sensitive to subtle overall relationships. A sign test should be more indicative in testing the hypothesis as it gives information on the sign of the z values independent of their magnitude. The proposed inverse relation would suggest a preponderance of negatively signed r_{TF} or z_{TF} values. Out of the 89 records, 52 have negative r_{TF} values. Using the normal approximation to the binomial test with continuity correction,

$$P(r^- > 52) \approx P(Z > \frac{51.5 - 44.5}{89/4}) \approx 7\%$$

where Z here means the standard normal distribution. This is borderline statistical significance.

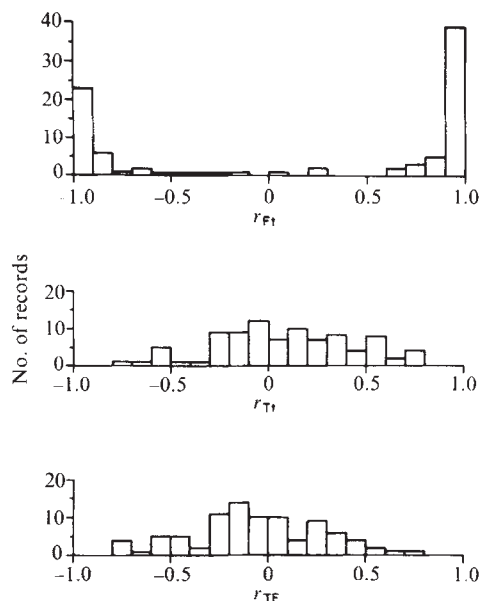


Fig. 2 Correlation coefficients of 89 records from 85 pairs of magnetic and weather observatories. Separate histograms are for the correlation between total magnetic intensity and time (r_{Ft}), temperature and time (r_{Tt}), and temperature and total intensity (r_{TF}).

Since geomagnetic intensity and climate are both regional parameters showing temporal and spatial coherence regardless of the hypothesis being considered here, data used to analyse the present hypothesis should be well distributed geographically. Otherwise, a spurious regional correlation between intensity and climate might unduly bias the overall results. Of the 89 records, 37 are from European stations although Europe covers only about 2% of the Earth's surface. The European z values were combined into a single datum by taking a weighted average of the individual values, using S_z^2 as weights. Similarly, six other pairs of closely spaced points were combined. The resultant Z_{TF}/S distribution is shown in Fig. 3. The χ^2 tests remain highly significant, but only 26 of the resultant 47 data have $r_{TF} < 0$, and $P(r^- > 26) \approx 23\%$, which is not statistically significant. Thus, based on the spread in the z_{TF} distribution and the results of the sign test, we conclude that the hypothesis that there is a historic negative correlation between local magnetic intensity and temperature trends is not statistically verified. The European data alone show an overall negative correlation significant at the

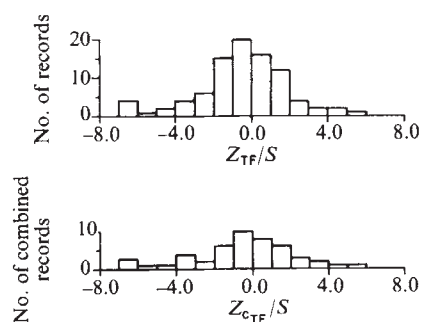


Fig. 3 The Fisher z transformation of the r_{TF} values of Fig. 1 divided by the standard deviations of the z distributions. Transformation of the raw values is given by Z_{TF}/S , while Z_{cTF}/S has combined values from stations in proximity to one another.

90% level, but because of the regional nature of the parameters, it would not be justifiable to conclude a causal relationship between intensity and climate.

Empirical studies of physical relationships are important for indicating appropriate lines of theoretical research. Although no proven theory links climate to geomagnetism, qualitative mechanisms have been postulated^{1,2,7,19}. These generally view the geomagnetic poles as flux tubes along which charged particles can propagate into lower regions of the atmosphere. Changing strength and orientation of the dipole field might then affect the quantity and geographic orientation of the ion corpuscular radiation⁷⁻⁹. However, it would be difficult to conceive of a mechanism that could link all local disturbances of the magnetic field to climatic change.

Because of the highly linear trends in magnetic field strength variation in many parts of the world, there will inevitably be some places where there will be an inverse relationship between the changes in temperature and the magnetic field. Without a viable physical mechanism that could produce such a relationship worldwide, we conclude that the weak, negative correlation between magnetic field strength and temperature in the historical record is a fortuitous result of the linear secular variation of magnetic field intensity and the poor geographic distribution of stations.

ROBERT S. STERNBERG
PAUL E. DAMON

Laboratory of Isotope Geochemistry,
Department of Geosciences,
University of Arizona,
Tucson, Arizona 85721

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Detection of Ni-Sn phases in temper embrittled Ni-Cr steels

A GENERIC problem in ferritic steels is the reduction in the grain boundary cohesive strength produced by the segregation of trace impurities of Groups IVB and VB of the Periodic Table. Such segregation results in a deterioration in mechanical properties produced, for example, by reversible temper embrittlement¹ and it also promotes enhanced susceptibility to a range of environmental enhanced cracking processes²⁻⁴. It has long been recognised that the embrittlement of steels is enhanced by the

Table 1 Composition of Ni-Cr steels studied

	C	Ni	Cr	Wt% Mn	Si	Mo	Fe
En30A	0.28	4.06	1.20	0.58	0.32	0.11	bal
BE10	0.18	3.33	1.42				bal
	S	Sb	p.p.m. P	Sn	As		
	80	40	130	140	240		
	20	<50	<50	425	<50		

Table 2 Distribution of particles at grain boundaries observed in En30A

Ageing time at 500 °C	Particles at grain boundary (%)		
	Fe ₃ C	M ₇ C ₃	Ni-Sn phase
≤100 h	100	—	—
500 h	96	2	2
5,000 h	80	15	5

presence of certain alloy elements, that is Ni, Cr or Mn, and measurements using Auger electron spectroscopy (AES)⁵⁻⁷ have demonstrated that specific combinations of alloy and impurity elements cosegregate to the boundaries, for example, Ni+Sn, Ni+Sb, Cr+P. These observations can be rationalised using Guttman's phenomenological theory⁸ which attributes the increased driving force for segregation to a positive interaction between alloy and impurity solute atoms. Previous investigations^{1,9-11} have failed to detect the presence of any three dimensional phase using conventional transmission electron microscopy and it is generally considered that the alloy element-trace impurity atoms form two dimensional phases at the boundaries. The only direct evidence for such alloy-impurity interactions and phase formation has been obtained using

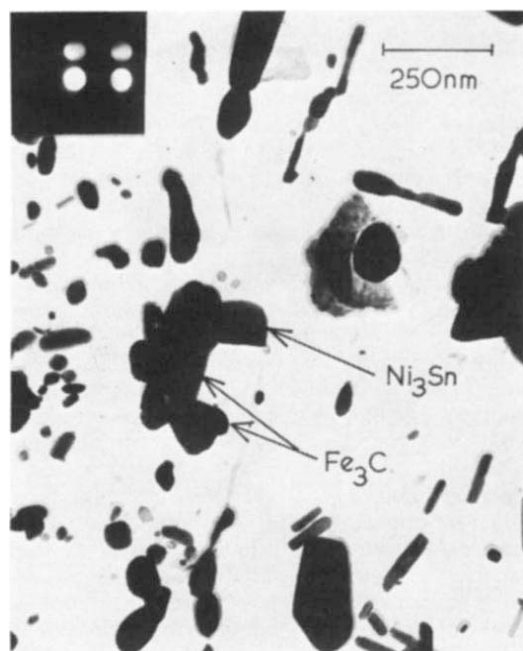


Fig. 1 Micrograph of an extraction replica showing the precipitate distribution in En30A steel after ageing for 5,000 h at 500 °C. A convergent beam microdiffraction pattern of the Ni₃Sn phase is shown in the inset, <211> zone.

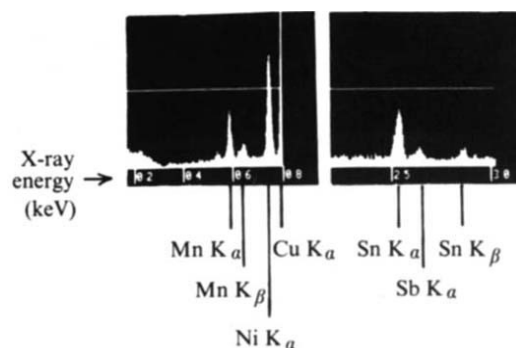


Fig. 2 Energy dispersive X-ray spectrum obtained from the Ni₃Sn phase indicating the presence of Ni, Mn, Sn and Sb.

Mössbauer spectroscopy^{12,13} but it is not possible using this technique to draw any positive conclusions regarding the location of such compounds. Measurements on a pure Fe-3.5 wt% Ni alloy doped with ¹¹⁹Sn¹² showed that Ni atoms associate preferentially with Sn atoms during tempering at 650 °C and the marked change in the spectrum observed on ageing at 500 °C, within the embrittling temperature range, is consistent with the formation of a Ni-Sn compound (Ni₃Sn₂ or Ni₃Sn₄). We report here some initial results from a detailed electron microscopic analysis of the microstructures of a commercial (En30A) Ni-Cr steel and a special laboratory made steel (BE 10) having similar Ni and Cr contents, but doped specifically with tin. The composition of the two steels is presented in Table 1. Both steels were oil quenched from 950 °C, tempered for 1 h at 650 °C and aged at 500 °C for times up to 5,000 h. Carbon extraction replicas, taken from polished surfaces were examined in a Philips EM400 microscope fitted with an energy dispersive X-ray system. Selected individual precipitate particles were analysed using convergent beam microdiffraction and X-ray analysis. Attention was focused on the particles which delineated the lath packet and previous austenite boundaries, that is, high-angle boundaries.

An electron micrograph showing the precipitate distribution in En30A after ageing for 5,000 h at 500 °C is presented in Fig. 1. The results from analysing the precipitates in the quenched and tempered condition and after ageing at 500 °C for times up to 5,000 h are listed in Table 2. For ageing times <100 h at 500 °C only orthorhombic Fe₃C particles, in which 10–20% of the iron is replaced by Cr, were identified. On ageing for 500 h at 500 °C the boundaries contained about 96% Fe₃C and 2% hexagonal M₇C₃ particles containing Fe and Cr in approximately equal amounts. In addition, about 2% of the particles were analysed to contain Ni and Sn and an example is arrowed in Fig. 1. Such particles were invariably located within small clusters of particles and were apparently attached to Fe₃C particles, although frequently M₇C₃ particles were also present in the clusters. The structure was analysed to be cubic with a lattice unit cell size of 0.598 ± 0.004 nm which matches a Ni₃Sn phase previously reported¹⁶ to be stable only above 950 °C. A <211> zone axis diffraction pattern is shown in the inset of Fig. 1. Two sections of the X-ray spectrum from a particle are presented in Fig. 2. It can be seen that the particles contain significant amounts of Sb and Mn in addition to Sn and Ni but no P was detected, although parallel AES studies¹⁴ have shown that P is present at the grain boundaries in significant amounts (~15 at %). The copper K_α peak in the spectrum comes from the copper grid on which the replica was mounted. On ageing for 5,000 h at 500 °C the proportion of M₇C₃ and Ni-Sn precipitates increased to 15% and 5% respectively, the balance again being Fe₃C.

A more limited study was made of the high purity laboratory made steel, BE10, doped with 425 p.p.m. Sn (Table 1). An examination of the material aged for 5,000 h at 500 °C showed two significant differences from the ageing behaviour exhibited

by En30A. First, a higher proportion of M_7C_3 was present suggesting that the transformation from Fe_3C is occurring at earlier ageing times. Second, a Ni-Sn phase was again observed but was only found adjoining M_7C_3 particles. Results from microdiffraction and X-ray analysis were consistent with the phase being of the type Ni_3Sn_2 having a hexagonal superlattice structure. Thus, these results provide further support of the Mössbauer results¹² which were also obtained from a special laboratory made steel having a high Sn content.

In the present study, convergent beam microanalysis¹⁵ has been used to characterise individual particles as small as 20 nm diameter in terms of both structure and composition. This technique has enabled us to obtain the first direct evidence for the formation of discrete three dimensional intermetallic compounds from alloy and impurity elements in temper embrittled steels. The results indicate a dependence between steel composition, carbide transformation kinetics and the type of Ni-Sn phase formed and therefore caution is required in predicting the behaviour of commercial steels based on the results obtained from specially prepared casts. The present work demonstrates that careful microstructural characterisation provides an important insight into the processes contributing to grain boundary centred embrittlement phenomena.

J. M. TITCHMARSH
B. C. EDWARDS
G. GAGE
B. L. EYRE

*Metallurgy Division,
Atomic Energy Research Establishment,
Harwell, Didcot, Oxon, UK*

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Electron nuclear double resonance signals from three ingredients of coal

ELECTRON spin resonance (ESR) spectra from coals were first reported in 1954¹⁻³. Since then many ESR studies on coals have been conducted, and the signals have been shown to arise from free radicals^{4,5}. Recently Kevan *et al.* studied coals using the electron nuclear double resonance (ENDOR) technique⁶. They detected a single-peak signal at the free proton frequency, thus providing the first direct evidence that the environments of radicals in coals contain hydrogen atoms. We report here on ENDOR signals from three ingredients which were separated out from a coal sample. The ENDOR signal from each ingredient is substantially different from that of the other ingredients. In addition, a proton hyperfine structure (hfs) was experimentally resolved, probably for the first time, for a radical in one ingredient of a coal sample.

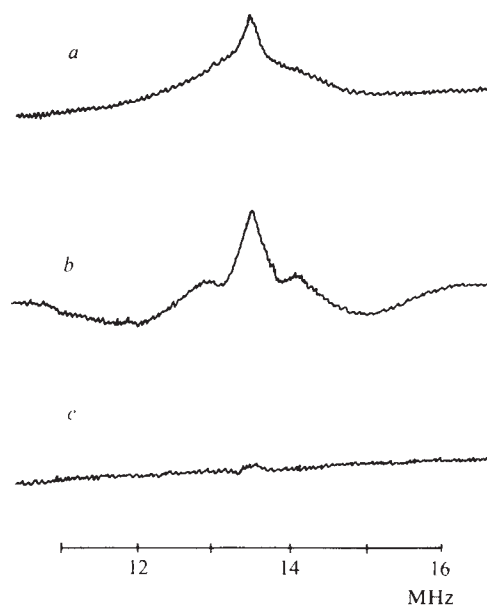


Fig. 1 ENDOR signals from three ingredients of a bituminous coal sample at a relative microwave power of -11 db. The signals were obtained for the out-of-phase mode⁸ at room temperature in air. The RF field was 8.2 G, which was measured by a sideband method¹¹. The free proton frequency was 13.5 MHz. *a*, Vitrain; *b*, clarain; *c*, fusain.

The sample studied was bituminous coal taken from a coal mine in Alabama. This sample showed three distinct layers—bright-glossy, satin-glossy and dull—composed of vitrain, clarain and fusain, respectively⁷. The coal sample was crushed into pieces of average size $0.5 \times 0.5 \times 1.0$ mm. Several pieces of each ingredient were then hand picked, and were put into a Teflon sample tube, which was designed to fit the ESR/ENDOR sample holder.

The ESR absorption from these ingredients showed only a small oxygen effect in intensity⁴. The radical concentration, which was measured using a DPPH probe, was comparable for the three ingredients: $4 \times 10^{19} \text{ g}^{-1}$ for vitrain, $4 \times 10^{19} \text{ g}^{-1}$ for clarain, and $3 \times 10^{19} \text{ g}^{-1}$ for fusain.

The ENDOR technique with an intense radiofrequency (RF) field⁸⁻¹¹ was used. The signal was detected at room temperature in the out-of-phase mode⁸ using an RF field of 8.2 G. Detection of the ENDOR signal was difficult when the RF field was below 3 G. No significant ENDOR signal was detected when the in-phase detection mode was used, even though the applied RF field was higher than 3 G.

The ESR signals from vitrain and clarain samples exhibited a maximum intensity at a relative microwave power of -11 db (-3 db for fusain samples), and the ENDOR signals from the three samples at -11 db are shown in Fig. 1. The signals are quite different for the three different coal ingredients. These spectra indicate that the environment of the unpaired electrons in each ingredient is substantially different from that of the other two.

The chemical compositions of the three ingredients are slightly different from each other⁷: the carbon content is highest and the hydrogen content lowest in fusain, while the content of volatile matter is highest in clarain. Thus it is possible that different ingredients contain different free radical species. Even if these radicals are chemically identical, the environment of the radical would differ from one ingredient to another. Note that the technique used here can effectively detect ENDOR signals from protons in the environment of a free radical¹⁰.

Furthermore, an out-of-phase ENDOR signal, such as that observed in the present case, is highly sensitive to the spin-lattice relaxation time⁸. Thus, it is quite possible that although the

radicals, including their environments, are chemically identical, different ingredients could give quite different ENDOR signals; this difference could originate from the difference in spin-lattice relaxation time, which is due to minor difference in structure deformation of the radical and/or its environment between different ingredients.

For these reasons the different coal ingredients might be expected to give different ENDOR signals. The nature of the difference in ingredients which is responsible for the difference in the ENDOR signals, however, is not yet clear.

The weak signal for fusain (see Fig. 1) can be attributed to an impurity. Examination of the specimen by a magnifier revealed a small bright region which appeared to be vitrain. Another specimen of fusain with a larger bright region showed a substantially stronger ENDOR signal.

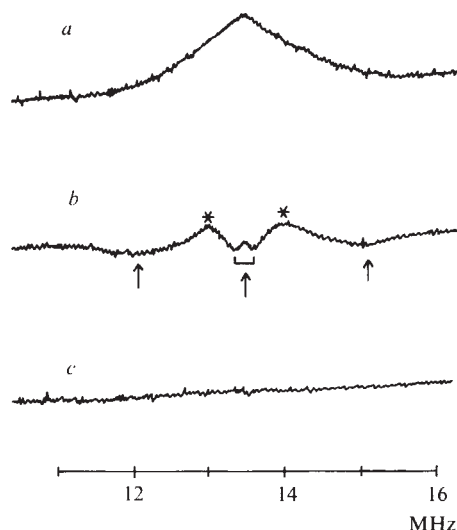


Fig. 2 Signals similar to those in Fig. 1 but observed at a relative microwave power of -20 db. The relative amplifier gain was twice that for the case of Fig. 1. *a*, Vitrain; *b*, clarain; *c*, fusain.

The sharp central peaks of the ENDOR signals from both vitrain and clarain, as seen in Fig. 1, exhibited a line width and line intensity dependence on microwave power which was very similar to the ENDOR signal reported by Kevan *et al.*⁶ The line width of our central peaks (~ 0.4 MHz) increased appreciably at higher microwave powers. The intensity of these peaks was also very sensitive to microwave power, and at a relative microwave power of -20 db (see Fig. 2), no evidence for this central peak is seen in vitrain and in clarain. In fusain this peak is very weak. This type of single ENDOR peak has been detected previously in irradiated crystals⁸. This peak and the similar one from these coal samples result from RF-field-induced positive phase shifts of the modulated ESR signals. In contrast, ordinary hfs ENDOR signals arise from negative phase shifts. The mechanism for the appearance of these single peaks has not been clarified.

As is seen in the second curve of Fig. 2, the ENDOR spectrum for clarain shows three negative signals (shown by arrows in Fig. 2), arising from hfs splitting. The central signal arises from protons with very small hfs, and the other two from proton couplings with an average hfs splitting of 3 MHz. The latter splitting is probably the first resolved case of hfs in coals.

It is not clear whether the two positive peaks for clarain (starred in Fig. 2) are hfs signals. It is possible that these two peaks appear as a result of a superposition of the signal from a vitrain impurity. In another specimen of clarain, at a microwave power of -20 db the position of the bottom of the central signal (negative in Fig. 2) was higher and in the positive region. This could indicate a higher concentration of a vitrain impurity which, when combined with the central negative signal, could give rise to the two observed positive peaks.

The signal from vitrain was broad and positive (see Fig. 2). Note that vitrain is known to have five modifications⁷. Thus, it is possible that this specimen could be further separated into five or more ingredients.

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ICHIRO MIYAGAWA
CHESTER ALEXANDER JR

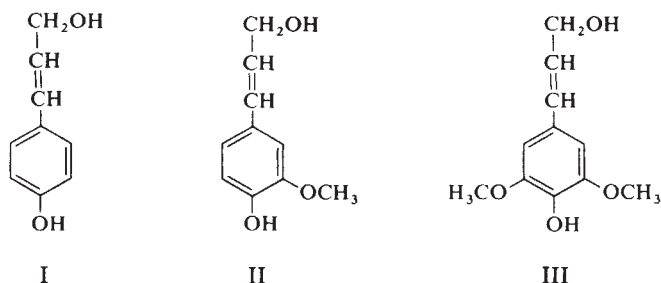
Department of Physics and Astronomy,
The University of Alabama,
University, Alabama 35486

Received 25 September 1978; accepted 2 January 1979.

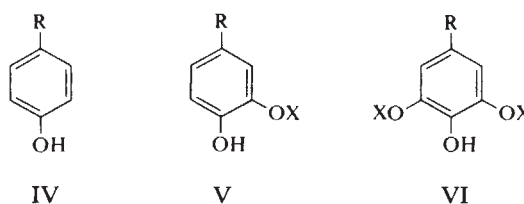
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Lignin-like polymers in coals

ALTHOUGH lignins are major constituents of plants from which coals are derived, their roles in the coalification process and resulting coal structures have not been defined. On the basis of chemical and biological degradations, lignins are considered to be polymers of *p*-coumaryl- (I), coniferyl- (II), and sinapyl- (III) alcohols¹⁻³.



Isolation of coal degradation products related to these three monomers should shed some light on the roles played by lignins during coalification and in the final structure of coals. To date such studies have had only limited success. Many oxidative degradations have also been carried out to break coal down into simpler species; however, isolation and identification of *p*-hydroxyl (IV), vanillyl (V), and syringyl (VI) groups, which are characteristic lignin oxidation products, have not yet been confirmed.



R = $-\text{CHO}$, $-\text{COCH}_3$ or $-\text{COOH}$; X = H or CH_3

Table 1 Summary of CuO–NaOH oxidation of coal*

Wt % of products†	Lignite	Bituminous
Organic acid (benzene–ether extract)	35.3	19.6
Humic acid-like material (methanol extract)	54.3	61.0
Non-oxidised coal	11.0	21.0
Wt % of organic acids‡		
Phenolcarboxylic acid	66.6	54.1
Benzenecarboxylic acid	26.0	36.2
Naphthalenecarboxylic acid	2.0	3.3
Heterocyclic acid	—	1.6
Aliphatic dibasic acid	2.6	1.0
Others§	2.8	3.8
Ratio of phenol/benzene carboxylic acids	2.56	1.49

* Elemental analysis (dry ash free basis): Lignite (Wyoming) C, 66.4; H, 4.7; N, 1.5; S, 1.1; O, 26.3 (by difference). Bituminous (Illinois #2) C, 73.9; H, 5.2; N, 1.4; S, 2.4; O, 17.1 (by difference). Anthracite (Pennsylvania PSOC 85) C, 91.0; H, 3.8; N, 0.7; S, 1.2; O, 3.3 (by difference). Samples were pretreated to remove solvent extractable trapped organic materials before analysis.

† Wt % was obtained from sample on a dry ash free basis.

‡ Wt % was obtained from each benzene–ether extract and the amount of each compound determined from GC.

§ Aromatic aldehyde, keto and unidentified compounds.

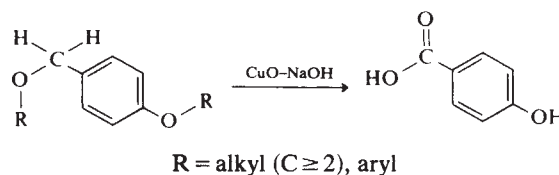
We report here that, using a CuO–NaOH oxidation procedure we were able to isolate phenolic acids which show a relationship between lignins and coals.

Although fulvic and humic acids^{4–6} have been degraded successfully with alkaline solutions to produce lignin-related phenols, this method has not been successful in coals^{7,8}. Reductive degradations of soil⁹ and coal-derived¹⁰ humic acids with sodium amalgam have been reported to produce a variety of phenolic compounds; however, the method used has been criticised because of its poor yields and its being so drastic that resulting chemical structures may have little or no relation to the starting material^{11,12}. In general, commonly used oxidants destroy phenolic rings or give complex products^{13,14}. Some of the oxidants such as nitrobenzene produce reaction byproducts that may interfere with the analysis of the oxidation products^{15–17}. To obtain lignin oxidation products from coals we used the alkaline cupric oxide oxidation method which has been successfully applied to analysis of lignins in plants¹⁵, humic acids^{17,18} and land-derived marine sediments¹⁵. To obtain detailed information of the CuO–NaOH oxidation, control experiments with known phenols, phenyl alcohols, aryl alkyl ethers and alkyl benzenes were also carried out using the procedure employed for the coals.

After removal of trapped organic compounds^{19,20}, 5 g of each coal sample (lignite, bituminous and anthracite, see Table 1) was oxidised with alkaline cupric oxide (52 g of CuSO₄ · 5H₂O, 37 g of NaOH and 185 ml of H₂O) at 200 °C for 8–10 h by the method of Greene *et al.*²¹. After separation of insoluble residue, the alkaline solution was acidified with HCl. The acidic solution was concentrated and extracted repeatedly with benzene–ether (1:3) and finally with methanol. Yields of the oxidation products from lignite and bituminous coals are shown in Table 1. No organic solvent-extractable oxidation product was isolated from anthracite coal. The benzene–ether extracts were derivatised with d₆-dimethylsulphate to yield d₃-methyl labelled derivatives. The derivatives were analysed by gas chromatography–mass spectrometry (GCMS) and high resolution MS using techniques described previously¹⁹. The retention times and mass spectra of the phenolic esters were confirmed with authentic samples. Gas chromatograms of the derivatives are shown in Fig. 1 with numbered peaks identified in Table 2. The methanol extracts from both coals were found to consist essentially of humic acid-like material soluble in alkali but complex and of high molecular weight^{3,22,23}.

As shown in Fig. 1 and Tables 1 and 2 the identification of large amounts of *p*-hydroxy- and 3,4-dihydroxybenzoic acids which are regarded as characteristic lignin oxidation products is very informative. Phenolic di- and tricarboxylic acids, and hydroxynaphthalenecarboxylic acids were also identified which are not obtained from land plants¹⁵ and marine sediments¹⁵ by the same oxidation.

All phenolic acids identified were found as OCD₃-derivatives. The mass spectra showed the complete absence of OCH₃-group. It is confirmed that no isotope exchange of the H in OCH₃ with D occurred during our procedures. Although partial demethylation reaction during the oxidation cannot be excluded from the control experiment, it is probable that the phenolic acids found were derived mainly from the fission of ether-linked aromatic systems as shown in the example below rather than from aryl methoxy units.



Indeed, it is known that very little or no methoxy groups are present in bituminous coals^{23,24}. In the lignite coal aryl methoxy groups are probably not present in significant amounts.

Trihydroxybenzoic acids (syringic group) were not found in the oxidation products. We have found that the authentic syringaldehyde was largely degraded by the oxidation. This may account for the fact that we did not observe them; however, the syringic group may have been degraded during coalification. Authentic mono- and dihydroxybenzenes were found to be stable in the same conditions.

Non-hydroxy substituted benzene-, naphthalene-, pyridine-carboxylic acids and humic acid-like materials found might be

Table 2 Organic acids identified as their d₃-methyl esters by GC–MS and high resolution MS

Peak no.	Compounds
1	Succinic acid
2	Methylsuccinic acid
3	Benzoic acid
4	Methylbenzoic acid
5	<i>p</i> -Hydroxybenzoic acid
6	Hydroxytoluic acid
7	1,2-Benzenedicarboxylic acid
8	1,4-Benzenedicarboxylic acid
9	1,3-Benzenedicarboxylic acid
10	Methylbenzenedicarboxylic acid
11	Dihydroxybenzoic acid
12	Pyridinedicarboxylic acid
13	3,4-Dihydroxybenzoic acid
14	Dimethylbenzenedicarboxylic acid, naphthoic acid (minor)
15	Hydroxybenzenedicarboxylic acid, 4-hydroxy-1,3-benzene-dicarboxylic acid (major)
16	Methyl-hydroxybenzenedicarboxylic acid
17	1,2,4-Benzenetricarboxylic acid
18	1,2,3-Benzenetricarboxylic acid
19	1,3,5-Benzenetricarboxylic acid
20	Methylbenzenetricarboxylic acid
21	Pyridinetricarboxylic acid
22	Naphthalenedicarboxylic acid
23	Hydroxybenzenetricarboxylic acid
24	1,2,4,5-Benzenetetracarboxylic acid
25	1,2,3,4-Benzenetetracarboxylic acid
26	1,2,3,5-Benzenetetracarboxylic acid
27	Methylbenzenetetracarboxylic acid
28	Dihydroxy-biphenyldicarboxylic acid (T)
29	Hydroxynaphthalenedicarboxylic acid
30	Benzenepentacarboxylic acid

(T) indicates that the identification is tentative.

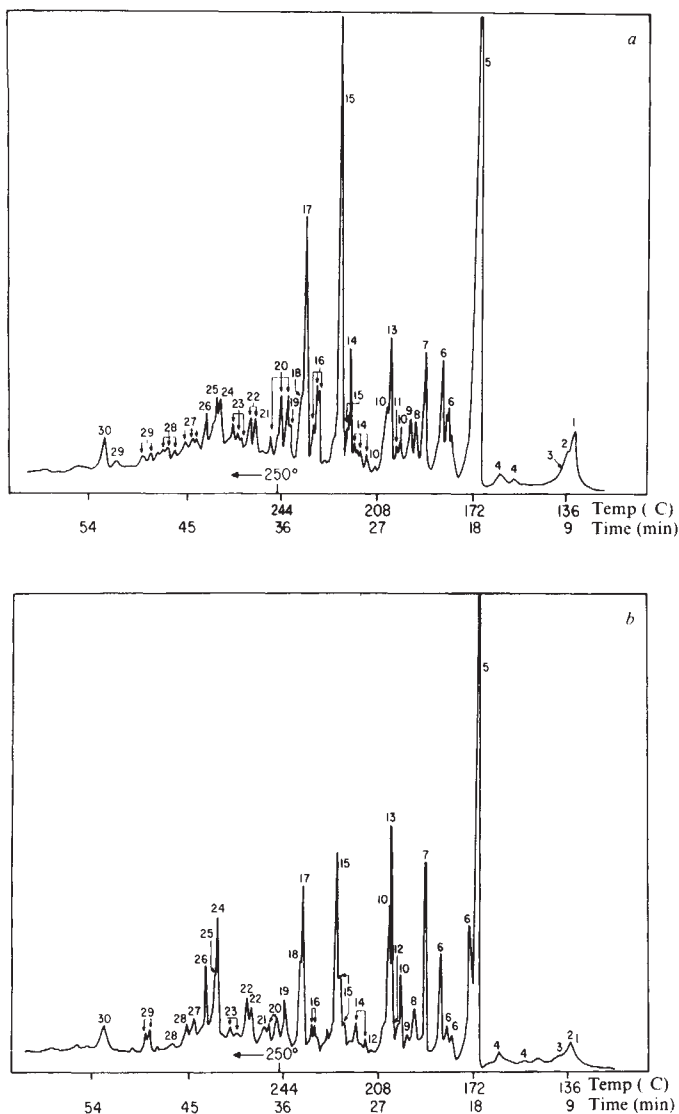


Fig. 1 Gas chromatogram of methyl esters of the organic acid fraction from lignite (a) and from bituminous coal (b). The analysis was carried out on a Perkin-Elmer 3920B gas chromatograph interfaced to a modified Bendix model 12 time-of-flight mass spectrometer with a variable split between a flame ionisation detector and the source of the mass spectrometer. The separation was made on a 15.2 m $\times$ 0.51 mm SCOT column coated with OV17 and temperature programmed from 100–250 $^{\circ}\text{C}$ at 4 $^{\circ}\text{C min}^{-1}$.

derived from non-lignin or extensively transformed lignin polymers. It is obvious that extensive transformation of lignin-like polymers occurred through several reactions^{22,25} such as demethylation, demethoxylation, dehydrogenation, oxidation, cleavage of ring structures and re-condensation during coalification from lignite to bituminous and anthracite coals. Indeed, we have observed that the CuO–NaOH oxidation of higher rank coals (C, 78–87%) produces very little phenolic acid with an increased yield of non-hydroxy aromatic and heterocyclic acids (data not shown).

Previously²⁰ we characterised aromatic acids trapped in lignite coal, and have found that these acids are qualitatively quite similar to those obtained from the present oxidation of the same pretreated coal. This indicates that the trapped acids were derived mainly from the hydrolytic oxidative degradation of lignin-like polymers during the coalification. Indeed, the later coalification product, the anthracite coal, did not give any trapped acid²⁰ or organic solvent-soluble CuO–NaOH oxidation product. Flaig has also discussed²² oxidative transformation of plant material during humification and coalification.

Our results show that lignin-like polymers are incorporated into the macromolecules of lignite and bituminous coals. It is indicated that the polymers contain more highly cross-linked structures with increased aromaticity compared with the original lignins. This is experimentally shown by the identification of phenolic polycarboxylic acids and hydroxynaphthalene-carboxylic acids which are not found in the CuO–NaOH oxidation products of lignins and plant materials. In addition, plant polyphenols other than lignin could also be precursors for coal formation.

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RYOICHI HAYATSU
RANDALL E. WINANS
ROBERT L. MCBETH

ROBERT G. SCOTT
LEON P. MOORE
MARTIN H. STUDIER

Chemistry Division, Argonne National Laboratory,
Argonne, Illinois 60439

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Fluctuations in SO₂ emission during recent eruptions of Etna

THE eruptive activity of July–August 1977 of Mt Etna consisted of four separate, short eruptive episodes: 16–22 July; 5–6 August; 14 August; 24 August. The rate of SO₂ emission during these eruptions was monitored using a remote-sensing correlation spectrometer. These data suggest that fluctuations in the SO₂ emission rate provide a means for predicting eruptions of Etna.

The July–August 1977 eruptions were confined to the North East Crater vent, while continuous degassing occurred from the central vents. The continuous gas plume was blown downwind immediately after it was emitted while the eruption plumes from the North East Crater rose 500–1,000 m before being blown horizontally. As the plumes moved downwind they were maintained at elevations ranging from 2,000 to 3,000 m above sea level. Monitoring began on 24 July, immediately after the first eruptive episode had ended, and continued until 27 August.

A Barringer Correlation Spectrometer (COSPEC) was used to monitor the SO₂ in the volcanic plume. (The application and design principles have been described by Newcomb and Millán¹.) The spectrometer consists of a Cassegrain telescope, a

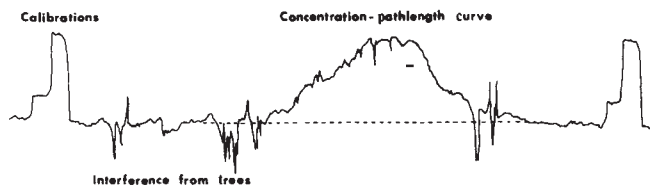


Fig. 1 A typical SO_2 plume cross-section obtained with the COSPEC mounted in an automobile. The instrument is calibrated at both ends of a traverse. The large calibration represents 370 p.p.m. m while the small calibration represents 125 p.p.m. m. The abscissa of the curve is time. A single traverse took ~ 15 min.

grating spectrometer, a correlator disk, and processing electronics section. Incoming radiation is dispersed by the spectrometer. The correlator disk then alternately passes UV spectra of high and low SO_2 absorption. This modulated signal is monitored by photomultiplier tubes which produce a voltage that is processed by the electronics. This processed voltage is proportional to the number of SO_2 molecules in the field of view of the instrument. This is a concentration-pathlength value and has dimensions in parts per million . metres (p.p.m. m). The response of the instrument is calibrated by reference cells filled with known concentrations of SO_2 . The instrument also has an automatic gain control to compensate for changes in the radiation source (the daytime sky).

Data reported here were gathered with the COSPEC mounted in a vehicle with a right angle mirror attached to the telescope to allow a vertical field of view. The vehicle was driven under the plume approximately perpendicular to the plume direction. Concentration-pathlength data from the COSPEC are recorded on a stripchart recorder along with odometer readings to provide a distance scale. Instrument calibrations were done before and after each traverse so that instrument drift could be checked. Figure 1 shows the record from a single traverse.

The area under the concentration-pathlength-distance curve represents a SO_2 cross-section of the plume (p.p.m. m^2). This value is corrected for traverses that are not at right angles to the plume direction. A mass flow of SO_2 (tonnes d^{-1}) is calculated by multiplying the p.p.m. m^2 value times the horizontal wind speed of the plume (m d^{-1}) times the density of SO_2 gas ($\text{tonnes per p.p.m. m}^3$).

The traverses were made on the roads shown on Fig. 2. The arrow from the summit shows the average wind direction for the period of this study, while the range of the wind direction

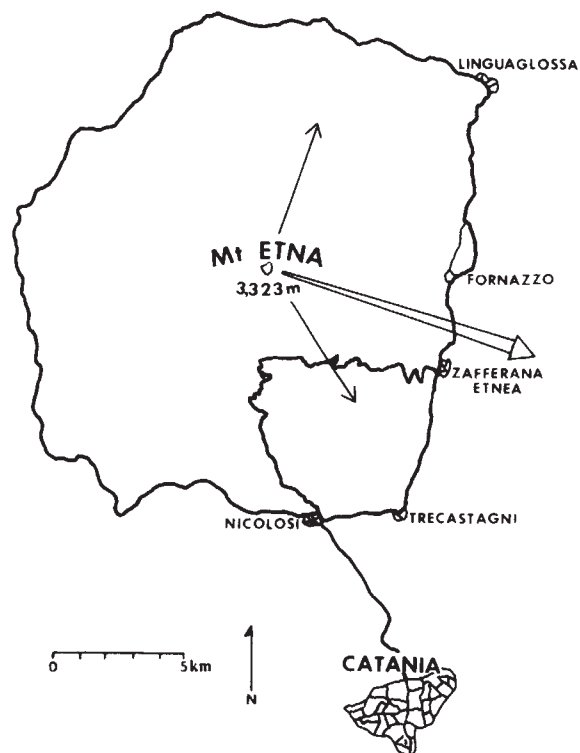


Fig. 2 Map of the roads used to obtain traverse data. The large arrow from the summit represents the mean wind direction while the smaller arrows represent the range of the wind directions.

for the period 16 July–28 August 1977 is shown by the shorter arrows. The roads are all between 7 and 10 km from the summit, and between 1,000 to 2,000 m below the plume as it passed overhead.

Two to eight traverses were made per day. A mass flow value was calculated for each traverse and the values from a given day were averaged to give a mean mass flow value for that day. Cloudy weather (clouds reduce the UV intensity and increase the instrument noise) or equipment failure account for missing days of data.

The activity of Mt Etna during July and August 1977 allowed the measurement of SO_2 output before, during, and after eruptive activity of the volcano. The SO_2 mass flow data are plotted

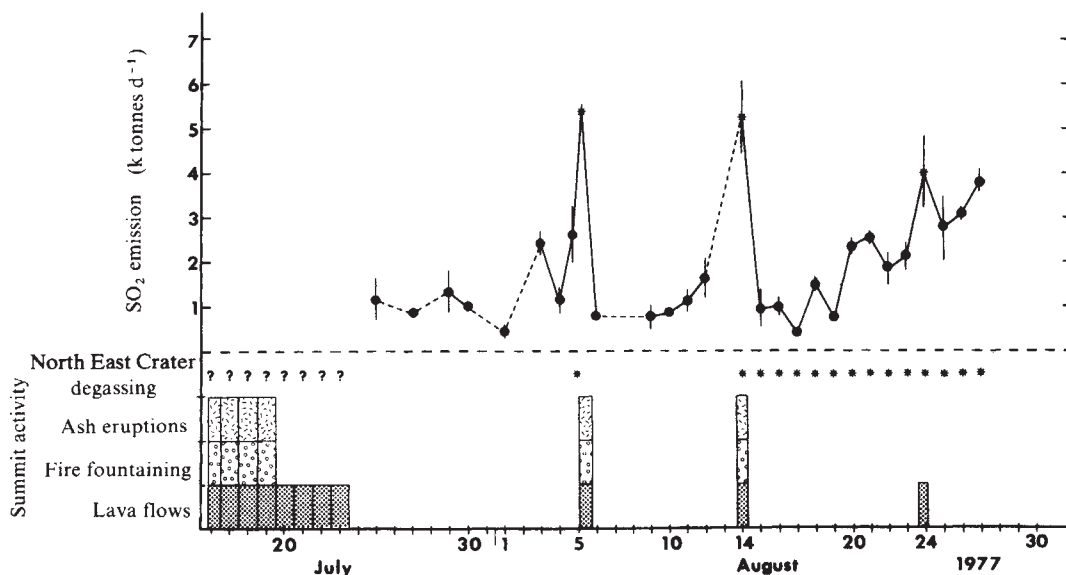


Fig. 3 SO_2 mass flow averages for the period of study. The dotted lines connecting the SO_2 data points are periods with missing days of data. The bars through each data point represent 1 standard deviation of the daily average. The bar graphs represent qualitative estimates of summit activity.

in Fig. 3. The SO₂ fluctuation curve is a plot of SO₂ mass flow (tonnes d⁻¹) versus time (16 July–28 August 1977). The bars through the points equal one standard deviation of the daily data. These should not be interpreted as error bars, but, rather an indication of the natural fluctuation of the volcano for a given day. The observed summit activity categories are meant only as qualitative estimates of the actual summit activity. The dotted lines connect intervals where there were missing days of data.

The data from 25 July to 1 August established a baseline level of ~1,000 tonnes d⁻¹ for SO₂ mass flow during non-eruptive periods. The traverses made on the morning of 5 August showed SO₂ levels up to 3.3 times the 1,000 tonnes d⁻¹ baseline value (1,830–3,330 tonnes d⁻¹). At 14.20 on the same day an eruption began in the North East Crater. Continued monitoring during the eruption showed that the SO₂ levels were above 5,000 tonnes d⁻¹.

By the morning of the 6 August, the activity had ceased and the SO₂ emission dropped to below the 1,000 tonnes d⁻¹ baseline level. The mass flow apparently remained constant until the 10th when a gradual increase began which continued through the 12th. Due to poor weather, the SO₂ levels were not measured on the 13th. At 03.00 on the 14th, the third eruptive phase began with SO₂ levels during the day again above 5,000 tonnes d⁻¹. By the evening of the 14th, the eruption had ceased and SO₂ levels on the 15th returned to the 1,000 tonnes d⁻¹ level.

SO₂ emissions from 15 to 24 August gradually increased to fairly high levels. The 4,000 tonnes d⁻¹ average on the 24th was accompanied by a small lava flow within the North East Crater, but no explosive activity occurred as in the previous eruptions. Monitoring ended on 27 August, with the SO₂ levels still fairly high. However, no further activity was observed until December 1977.

The SO₂ mass flow values reported here are all minimum values. Three factors combine to reduce the measured SO₂ value. These factors are signal attenuation, plume opacity and loss of SO₂ by reaction to SO₄. The COSPEC is a pathlength dependent device. As the distance between the plume and sensor increases, the measured SO₂ values decrease. This has been described by Moffat and Millán² in studies of powerplant plumes. Increasing plume opacity also tends to reduce the measured SO₂ values. This has also been documented in powerplant plume studies³. This factor is important to consider during eruptive activity when there is an increase in pyroclastic material in the plume. A third mechanism for SO₂ signal reduction is the reaction of SO₂ to SO₄, in water droplets in the plume. The COSPEC 'sees' only SO₂ therefore, SO₂ converted to SO₄ is effectively lost. The fact that these values are minimums does not reduce the validity of the SO₂ fluctuation curve since the mass flow averages were all obtained using the same technique, at equal distances from the vent, and at equal vertical distances from the plume.

It is evident that the increases in the SO₂ mass flow from Mt Etna could possibly be used as premonitory signs of impending activity. Two possible types of increases are suggested. First, the sharp increase such as before the 5 August eruption. Second, the gradual increase as exemplified by the periods 9–12 August, and 15–24 August.

The SO₂ mass flow variations correlate well with a model for the movement of magma within the conduit system at Etna proposed by Wadge⁴. He suggests that the eruptive pattern of Etna begins with filling of the conduit system. Activity is initiated by summit eruptions which are followed by flank eruptions. After the flank eruption drains the conduit system, the eruption ends and the cycle begins again. The SO₂ emission levels directly reflect the different levels of the magma.

In theory similar patterns should exist for all volcanoes. As the magma rises, SO₂ gas will be released in greater quantities. One should be able to monitor this increase. The absolute values for mass flow will vary from volcano to volcano, and possibly from eruption to eruption. The time fluctuation between observed

SO₂ increase and eruptive activity could be either compressed or extended.

Previous studies of condensates from high temperature fumaroles adjacent to main volcanic vents^{5–7} has long suggested that the SO₂ volume or S/Cl ratio increases before eruptive activity. The technique of remote sensing by correlation spectrometer provides a simpler and safer method for monitoring the SO₂ emission from the main vents of a volcano.

Several investigators have measured the SO₂ emission from various volcanoes^{8–12}. These data are in the same range as the data collected at Mt Etna, however, much more data are needed from individual volcanoes to determine the range of SO₂ emission and time period fluctuations. Given sufficient data, patterns might become well enough established to allow prediction of eruptions based on SO₂ fluctuations.

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LAWRENCE L. MALINCONICO, JR

Department of Earth Sciences,
Dartmouth College,
Hanover, New Hampshire 03755

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Intra-plate deformation: a quantitative study of the faults activated by the 1978 Thessaloniki earthquakes

THE Thessaloniki region suffered from damaging seismic activity during the summer of 1978. During both major shocks that took place on the 24 May (*M*, 5.5) and 20 June (*M*, 6.5), manifestations of faulting could be observed in the epicentral areas. They showed that the continental crust is stretching out in this region. Measurement of slips on the active faults make it possible to compute the direction of traction (165° N) in the crust. These active faults show close kinematic similarities to previously neotectonically defined faults of Recent Quaternary age (after 400,000 yr) within the same region. Thus, study of Recent neotectonic faults often gives a correct prediction of the direction of possible slipping on faults which could be activated by earthquakes in the same region. We show here that a quantitative study of active and neotectonic faults demonstrates that in this intracontinental situation faulting deformation is satisfactorily explained with a model of continuous deformation.

The epicentral region is made up of a graben (Fig. 1) which demonstrates the presence of recent extensional tectonic activity in this area^{1,2}. An examination of the LANDSAT 1 satellite photographs (Fig. 1) indicates that this angular crank-shaped graben probably results mainly from sinistral wrench movement along NW–SE orientated fault zones. In addition opening occurs along normal faults of E–W to ENE–WSW directions. Similar movements have been observed, SW and SE of the Langadha Lake, on neotectonic faults which have a post Middle Pleistocene age³.

The faults that were formed during both 1978 earthquakes (Fig. 2) confirm this structural setting. Open cracks with an

oblique shear component, having centimetre-sized openings, have been observed in unconsolidated sediments of Upper Miocene–Pliocene and Quaternary ages. These cracks have a shear movement which is essentially dip-slip along planes which strike between 060° N and 090° N (maximum downthrows of 20 cm, see sites 32, 33 and 16 in Fig. 2) or sinistral strike slip (maximum displacements of 10 cm, see site 17) but have an important vertical component along faults striking between 110° N and 130° N. On the other hand, in bed-rock composed of Palaeozoic mica schists (sites 10, 11, 13, 15, 30, 35) the faults contain striations along their shear planes and these have a clear tectonic origin.

These faults and cracks are distributed throughout the epicentral zone and apart from some typical gravitational slide-failures (sites 23 and 26), they are located within Plio–Quaternary fault zones (Fig. 2) which have obviously been reactivated during the 1978 earthquakes.

Numerous cracks observed in unconsolidated sediments appear as small reverse faults. This could have been wrongly interpreted in terms of a compressional mechanism. Slip movement on the active fault plane only takes place where a required overburden pressure is reached (Fig. 3). In the shallow unconsolidated sediments, the motion is made possible by the opening of a curved crack the dip of which can be overturned at the surface. Thus the slip along the normal fault at depth manifests itself on the surface by a crack that has an apparent reverse fault geometry (Fig. 3). All the observed fault planes have a normal faulting component which corresponds to an active extensional tectonic activity.

Along the fault planes affecting the bed-rock, the slip vector corresponds to a frictional striation which can be exactly measured. In unconsolidated sediments of sufficiently rigid quality, the cracks at the surface yield the slip vector for the deeper active fault. This slip vector is equal to the displacement joining two points of contiguous origin being presently at opposite position across the open crack (Fig. 3).

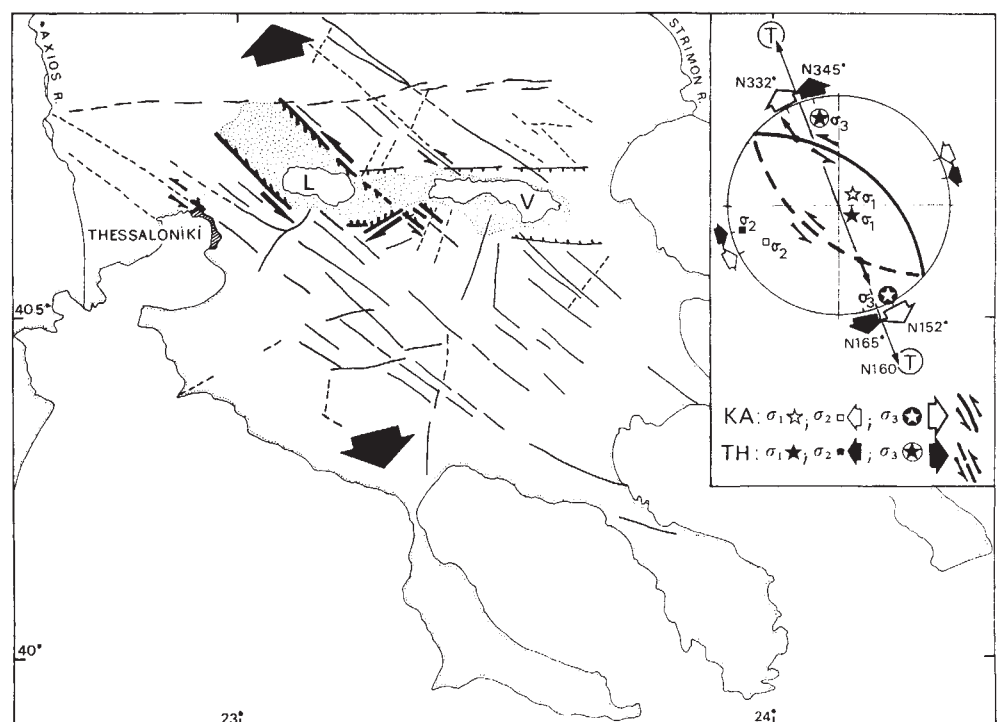
Open cracks could have been wrongly interpreted in terms of gravitational sliding. In such a case, slip-vectors have to be parallel to the steepest slope of the cracks which means that $\theta_{sv} \propto \alpha_{FP}$ (Fig. 4). This linear relationship gives the straight line in Fig. 4. The data, represented by dots, do not agree. The azimuths of the slip vectors lie between 300° and 30° N or

between 140° and 210° N. These two bands are in opposite position on the stereonet; in other words, the cracks cannot have a gravitational origin.

Figure 2 shows that the faults striking between 110° and 130° N have a sinistral–normal behaviour; those between 060° and 090° N are essentially normal and those between 015° and 040° N are also strike-slip but dextral–normal. These faults which are regionally scattered and which have a variable strike direction are a demonstration of the extent of the regional deformation which has taken place.

Regional deformation occurs through finite slip along existing fault planes in a highly fractured medium. In this situation, a model of continuous deformation ought to be tested. The slip $\vec{S}$ on the fault plane has to be parallel to the tangential component $\vec{\tau}$ of the applied stress. The orientation of $\vec{\tau}$ is only function of the orientation of the principal axes and of the ratio $R = \sigma'_1 - \sigma'_3 / \sigma'_2 - \sigma'_3$ where the σ' are deviatoric stresses⁴. Also the knowledge of the slip vectors $\vec{S}$ corresponding to a given family of faults can lead to the determination of the above parameters⁵. The computation searches for the parameter values which minimise the deviation between the predicted striations $\vec{\tau}$ and the observed striations $\vec{S}$ for all the fault planes. Four of the planes would be sufficient to compute a solution. The numerical procedure minimises the function $F = \sum_{k=1}^N (\vec{\sigma}'_k \cdot \vec{u}_k)^2 / |\sigma'_k|^2$ where σ' is the deviatoric stress acting on the fault plane, $\vec{u}$ the vector in the fault plane at right angle with the striation $\vec{S}$, and N the number of observed faults. Such a calculation was performed using only the most accurately measured slip vectors namely 10 bed-rock data from sites 10, 13, 15, 35 and from an additional site close to the sea, as well as three unconsolidated sediment data from sites 16, 22 and 23. This computation shows that the kinematics of active faults is best explained by a horizontal tension σ'_3 of azimuth 165° N. The intermediate stress σ'_2 is compressive, although of much smaller magnitude than σ'_1 and σ'_3 ; its orientation is horizontal with azimuth 76° N. This result gives a deviation between the observed $\vec{S}$ and predicted $\vec{\tau}$ which remains less than 15° . The situation is almost as satisfactory if the computation includes the other eight data from sites 2, 8, 17, 22, 32, 33, also in unconsolidated sediments but of lesser quality. Thus intra-continental tectonics can be satisfactorily analysed in terms of a continuous deformation model. This is because the geological material often contains numerous zones

Fig. 1 Structural map of the epicentral zone: lineaments observed from LANDSAT 1 pictures (thin lines), seismic and neotectonic faults described in this study (thick lines), graben of the Langadha (L) and Volvi (V) lakes (shaded area). Inset, the Wulff stereonet (lower hemisphere) shows the Quaternary fault (KA) to south-west of Langadha Lake and the major seismic fault (TH) with corresponding principal axes. Computed tensional directions are seen to coincide; black, white and T arrows correspond respectively to 1978 faults, Upper Quaternary faults and regional earthquake mechanisms.



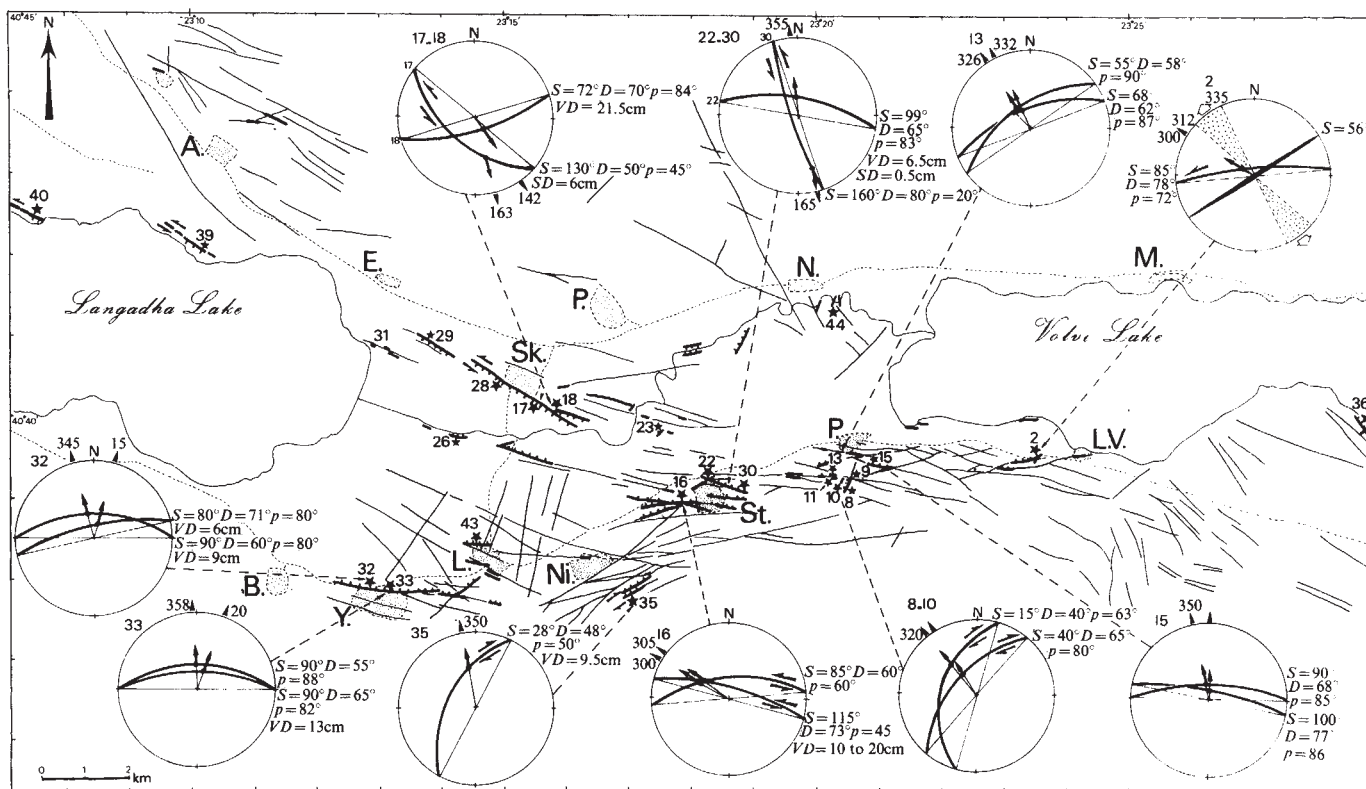


Fig. 2 Detailed map of the epicentral region: lineaments and faults observed on aerial photographs (thin lines), 1978 faults and cracks (thick lines hatched on the side of downthrows). Numbered stars refer to studied sites. Corresponding stereonet illustrate the strike (S) and the dip (D) of faults, the pitch (p) of the slip-vector (angle between the striation and the horizontal in the fault-plane), the vertical (VD) and horizontal (SD) components of the displacement (D). The arrowhead outside the circles indicates slip-vector orientation while the dot on the trace of the fault plane represents the slip vector, the attached arrow indicating the motion of the upper block. Letters are for the villages of Yerakarou (Y), Nikomedinon (Ni), Stivos (St), Skolario (SK) and Peristeronas (P).

of weakness. A striking feature of our analysis appears in the stereonet in Fig. 1; the tensional direction σ'_3 (thick black arrow) deduced here is compatible with the tension (thin arrow marked T) deduced from regional focal mechanisms^{2,6-8}.

A fault system postdating Middle Pleistocene forms the SW edge of the Langadha Lake graben (Fig. 1). The faults of direction 130° N are normal with sinistral strike-slip³; they have not moved during the 1978 earthquakes. The neotectonic slip-vectors on the various fault planes are compatible with a deviatoric stress tensor very similar to the one obtained for the active faults. In particular, the corresponding tension axis σ'_3 (thick white arrow on the stereonet in Fig. 1) lies close to the tensional orientations described above. Another fault zone (site 35, Fig. 2) was studied before the earthquakes; it was active this time (Fig. 3). The faults are normal with dextral strike slip and the 1978 slip vectors are of similar orientation as those corresponding to earlier motions.

About 500 m to the north of site 35, along the same fault zone, the bed-rock exhibits a mylonite structure in which the fracture planes often contain two different superposed sets of striations. Here the kinematics are quite different from those of recent and active faults; this implies a tension σ'_3 along $120-130^\circ$ N. The faults are probably younger than Mio-Pliocene but older than the Recent. These remarks indicate that only recent neotectonic features can be used to picture the present day active tectonics. It means that, for the region under study the regional stress field has not changed much during the recent period, probably Upper Quaternary, and that the material has not suffered much rotational deformation.

The analysis of ground motions during the last Thessaloniki earthquakes as well as the study of neotectonic data for Upper Quaternary faulting^{3,9} justify the use of models of continuous

deformation to account for intracontinental tectonics. This applies to strains of small magnitudes and hence short geological time periods. The integration over a longer time will give large scale continental deformations which can also be seen in terms of continuous deformation. Such an approach with the use of slip-lines has already been suggested¹⁰. For the Aegean region, the slip corresponds essentially to the dextral strike slip faults of

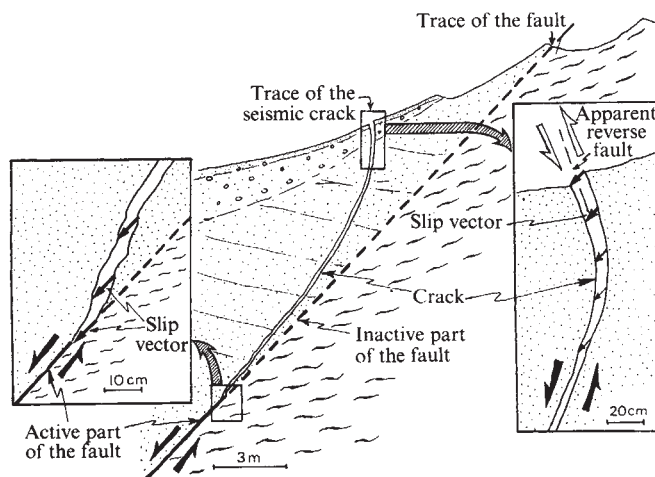


Fig. 3 Field view of the active Nikomedinon fault (site 35 on Fig. 2), sketched after visual observation of a subvertical erosion surface. Insets show detailed slip vectors.

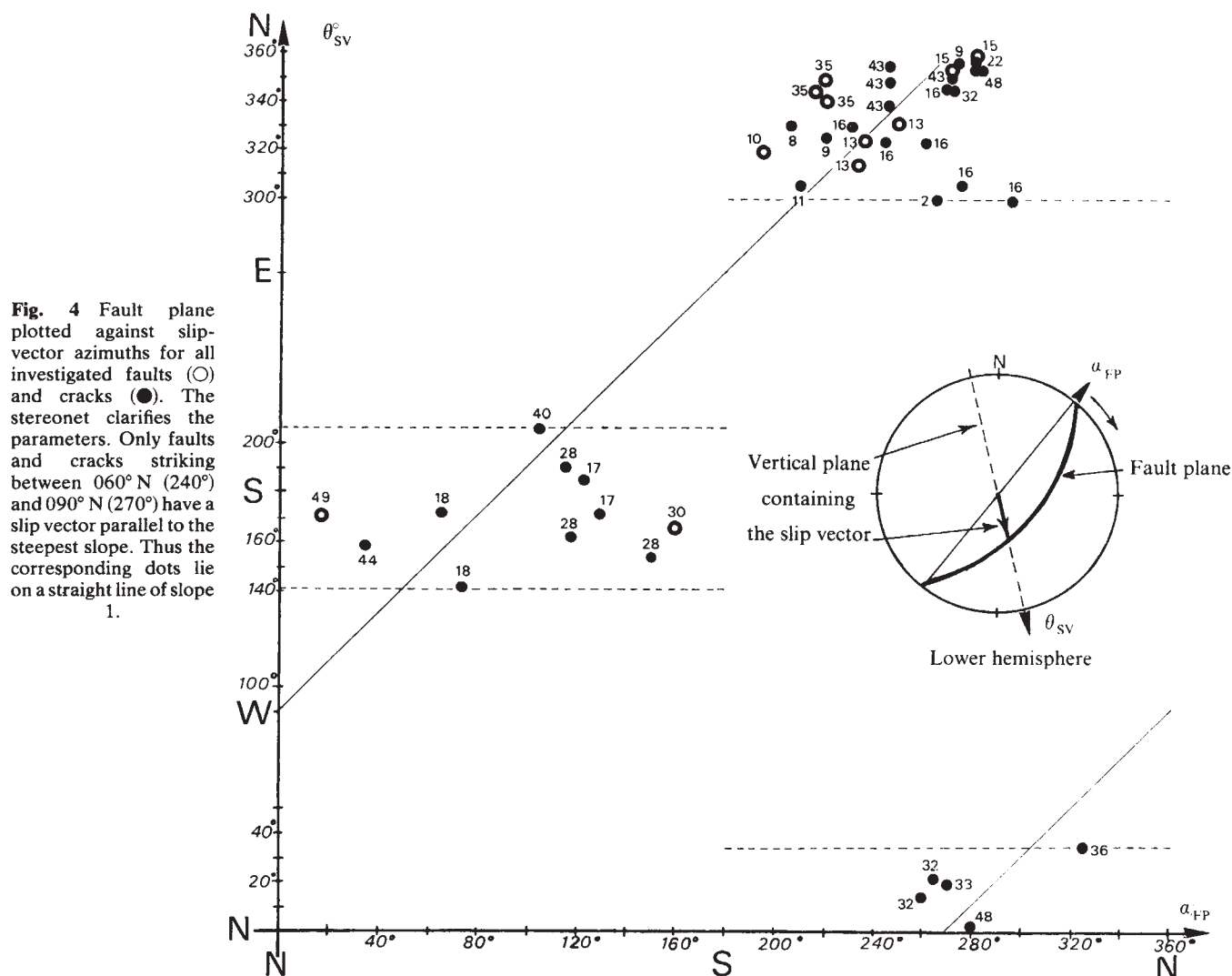


Fig. 4 Fault plane plotted against slip-vector azimuths for all investigated faults (○) and cracks (●). The stereonet clarifies the parameters. Only faults and cracks striking between 060° N (240°) and 090° N (270°) have a slip vector parallel to the steepest slope. Thus the corresponding dots lie on a straight line of slope 1.

NW Anatolia and to the sinistral faults of Macedonia^{3,11}. The deformation is, however, not purely planar so that extensive graben formation^{2,3} occurs near Thessaloniki.

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J. L. MERCIER

*Laboratoire de Géologie Dynamique,
Université de Paris-Sud,
91 405—Orsay, France*

N. MOUYARIS,
C. SIMEAKIS,
T. ROUNDYANNIS,
C. ANGELIDHIS

*Institute for Geology and Mining Research,
Messogeion Street 70,
Athens, Greece*

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A doughnut-shaped pattern of seismic activity preceding the Shimane earthquake of 1978

AN earthquake occurred inland at shallow depth in Shimane, southwestern Japan (Fig. 1), on 3 June 1978 GMT. Its seismic magnitude was estimated as 6.1 by the Japan Meteorological Agency (JMA), which was larger than the body wave magnitude of 5.1 estimated by the US Geological Survey, (USGS). Some damage to construction was caused in the epicentral region. We show here that the doughnut-shaped active area and seismicity gap in its centre immediately preceding the earthquake indicated its region and likely magnitude.

The possibility of an earthquake occurring was inferred from the following reasons. First, the sequence of earthquakes during the past 50 yr suggested that a major earthquake with magnitude about 5 might occur in this region within a few years¹ (S. Asano, personal communication). Figure 2 shows that major events with magnitudes about 5 took place successively during 1950–55 and 1963–66. After a 10-yr quiescent period, an earthquake of $M = 5.3$ (JMA) occurred on 1 May 1977, adjacent to the focal region of the most recent event of $M = 6.1$. Considering that the active-inactive periods had already occurred twice in this region, the $M = 5.3$ event was suspected to indicate the onset of another seismically active period for a few to several years.

Second, the aftershock activity of the $M = 5.3$ event in 1977 had reached a new stage. Locations of the aftershocks of the $M = 5.3$ event had been limited to a small area until the autumn of 1977 when the microseismically active area started to spread into the adjacent areas (Fig. 3). Slight activity took place in the area north-west (October–November), east (November–December) and south (January–February) of the $M = 5.3$ event. Furthermore, from the beginning of 1978, the activity extended southwestwards to a somewhat broader area. No earthquakes had been observed in that area during the preceding half year. Although such changes in microseismicity do not always represent the possibility of the occurrence of a major earthquake, the fact that this region had entered a new stage in seismic activity may be significant from the viewpoint of earthquake prediction. For example, some of the major events of the Matsushiro earthquake swarm in 1965–67, central Japan, were preceded by the spatial extension of microseismicity².

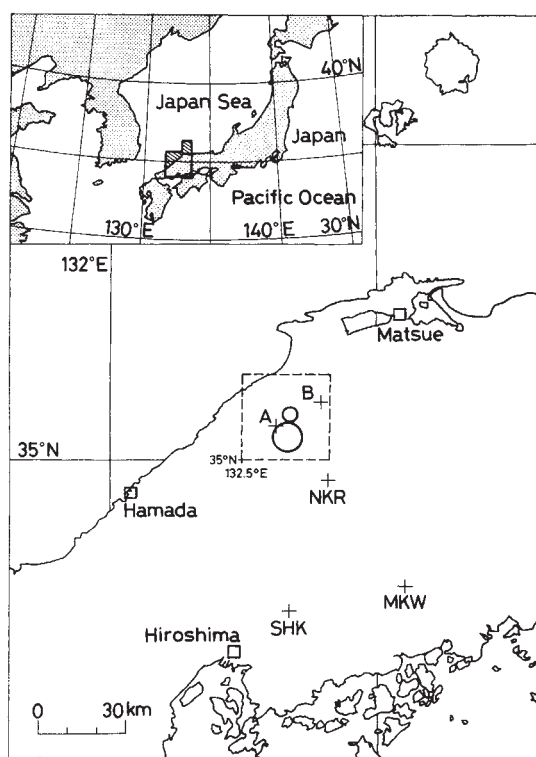


Fig. 1 Location of the $M = 6.1$ (larger circle) and $M = 5.3$ (smaller circle) earthquakes which occurred on 3 June 1978, and 1 May 1977, respectively. Crosses show the seismograph stations of the Shiraki Microearthquake Observatory (SHK, MKW and NKR) and the temporary stations (A and B) installed just before the $M = 6.1$ event. The area enclosed by the broken line is referred to in Figs 2 and 4.

Third, a doughnut-shaped pattern in microearthquakes had become apparent over a period of 5 months (Fig. 4). The microearthquakes which extended over the area south-west of the focal region of the $M = 5.3$ event had formed a doughnut-shaped active area and had left a circular aseismic area in its centre, suggesting a future earthquake, because such a pattern of seismicity had been seen before several other large earthquakes^{3,4}. The aseismic area was about 6 km in diameter. The extension and duration of this seismicity gap were considered to be too small for an event of $M = 7$ and probably 6, but large enough for a $M = 5$ event^{5,6}. The $M = 6.1$ event actually filled

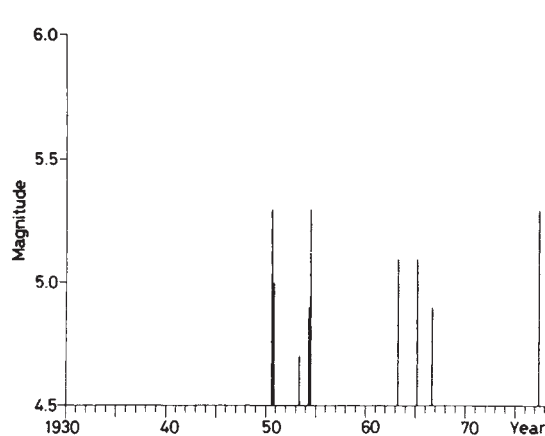


Fig. 2 A sequence of major earthquakes ($M > 4.5$) occurred within the area shown in Fig. 1 during the past 50 yr (data recorded after the Japan Meteorological Agency¹).

this seismicity gap, although the principal rupture seems to have started within the doughnut-shaped active area.

These seismological data which suggested the possible occurrence of major earthquakes were presented to the Coordinating Committee for Earthquake Prediction, Japan. The earthquake sequence of the last 50 years in this region was reported by JMA in the 37th committee, which was held in May 1977, just after the $M = 5.3$ event. After the aftershock area of this $M = 5.3$ event began to expand, the Shiraki Microearthquake Observatory reported this to the 40th and 41st committees in February and May 1978, respectively. These reports did not explicitly predict a major earthquake because of the lack of successful predictions in Japan.

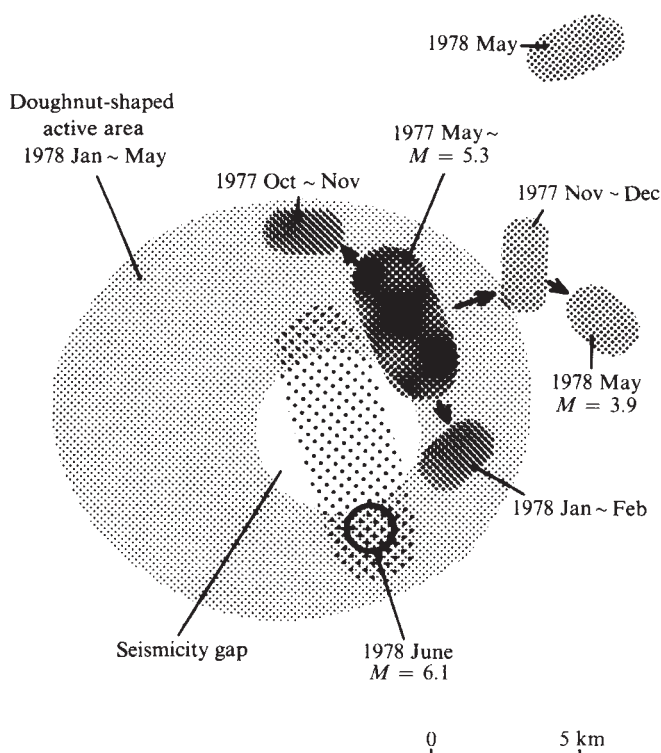


Fig. 3 Map of recent seismic activity around the focal region of the $M = 6.1$.

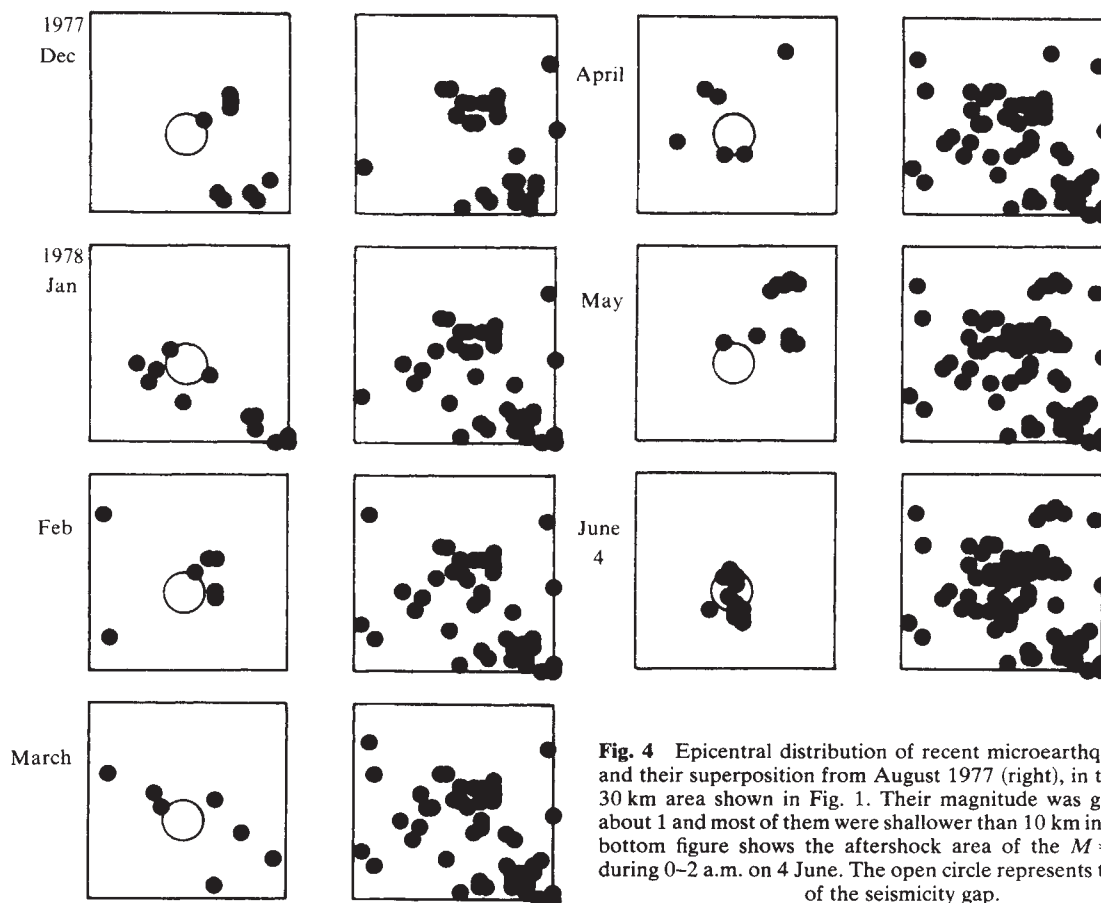


Fig. 4 Epicentral distribution of recent microearthquakes (left) and their superposition from August 1977 (right), in the 30 km $\times$ 30 km area shown in Fig. 1. Their magnitude was greater than about 1 and most of them were shallower than 10 km in depth. The bottom figure shows the aftershock area of the $M = 6.1$ event during 0–2 a.m. on 4 June. The open circle represents the location of the seismicity gap.

Because of this anomalous seismic activity, we carried out temporary observations with seismometers enclosing the area of the seismicity gap from 5 to 2 days before the $M = 6.1$ event. The temporary stations A (29 May–1 June) and B (30 May–1 June) are shown in Fig. 1. During this period, no events greater than about 0 in magnitude were observed in either the aseismic area or the doughnut-shaped active area. No events greater than about 1 in magnitude were also observed by the local network of the Shiraki Microearthquake Observatory until the $M = 6.1$ event occurred on 3 June. Thus, the focal region of the $M = 6.1$ event seems to have remained fairly inactive for several days just before the main shock.

The present $M = 6.1$ Shimane earthquake was preceded by anomalous seismic activity in both time and space. A doughnut-shaped active area and a seismicity gap in its centre were apparent 5 months before the $M = 6.1$ event. This suggested the location and the magnitude of the impending earthquake. The actual magnitude ($M = 6.1$, JMA) was larger than expected ($M = 5$). But a lesser magnitude ($M = 5.1$) was also presented by USGS. The low activity around the focal region for several days just before the main shock, which was determined by the temporary observations, might have represented the imminent outbreak of the $M = 6.1$ event. The microseismicity preceding the present event may be of interest as a way of predicting earthquake occurrence.

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KEN'ICHIRO YAMASHINA
YOSHIHIRO INOUE

Shiraki Microearthquake Observatory,
Earthquake Research Institute, University of Tokyo,
Shiraki, Hiroshima 739-14, Japan

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Parallel changes in species diversity and palaeotemperature in the Lower Miocene

OXYGEN isotope analysis of planktonic foraminifera from 12 samples of deep-sea sediment of Lower Miocene age has been performed to estimate the sea-surface temperature at that time, and the results are reported here. The samples were taken from a borehole in the English Channel, 110 km south-west of the Isles of Scilly¹. The planktonic foraminifera have already been described in detail¹ and it has been shown that the fauna is from the Lower Miocene Burdigalian *Globigerinoides trilobus* zone. Comparison with samples of the same age from New Zealand suggests sea-surface temperatures in the range 16–20°C or warmer, on the basis of oxygen isotope determinations in New Zealand² and on the presence in the present samples of *Globigerinoides sacculifer* (Brady). It has also been suggested from previous palaeoecological work on foraminifera of the Western Approaches, that there was only a small difference between the Miocene and present-day sea-surface August temperatures (which are 15–18°C)³.

In the ocean both plankton tow⁴ and isotopic⁵ data suggest that *G. sacculifer* inhabits surface water and is therefore a suitable species to analyse for the estimation of isotopic palaeotemperatures. Specimens were selected from the samples

Table 1 Oxygen and carbon isotope data for *Globigerinoides sacculifer* (Brady) from the borehole

SA2 Sample	$\delta^{18}\text{O}$ PDB	$\delta^{13}\text{C}$ PDB
1700	-2.18	+2.48
1701	-1.89	+2.74
1702	-2.23	+2.93
1703	-1.86	+2.34
1704	-1.57	+2.49
1705	-1.63	+2.36
1706	-1.50	+2.59
1707	-1.86	+2.75
1708	-1.05	+1.26
1710	-1.02	+2.24
1711	-0.90	+1.80
1712	-0.99	+1.79

examined by Jenkins¹ and roasted *in vacuo* at 400 °C for 30 min before isotopic analysis, to volatilise possible organic contaminants. Carbon dioxide was generated by the action of orthophosphoric acid at 50 °C and analysed isotopically in a VG Micromass 602C mass spectrometer⁶. Overall reproducibility of the measurements is about $\pm 0.1\%$ (1σ). Measurements of δ_c (Table 1) have been converted to sea-surface temperatures assuming an oxygen isotopic composition δ_w for the Lower Miocene ocean, before the accumulation of the Antarctic ice sheet, of -1.2% on the PDB scale⁷. Although there is some uncertainty in the temperature derived using this estimate it probably does not exceed one or two degrees and is unlikely to contribute to the variability among the estimates. Temperatures were estimated using the temperature- δ relationship: $T = 16.9 - 4.4(\delta_w - \delta_c) + 0.1(\delta_c - \delta_w)^2$ (refs 8, 9). Figure 1 shows error bars corresponding to ± 2 standard deviations; these do not take account of possible systematic errors which would not contribute to the observed temperature changes within the sequence.

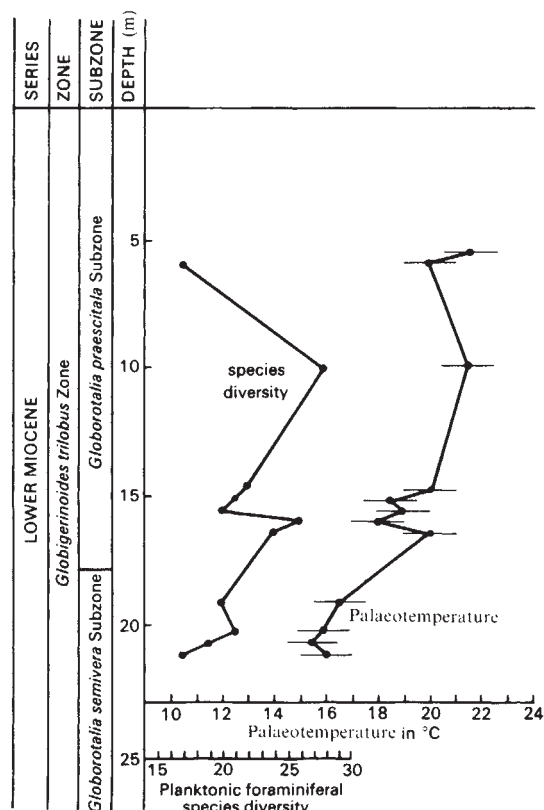


Fig. 1 Measurements of palaeotemperature and species diversity of planktonic foraminifera in the Lower Miocene Sea Lab Trial borehole from the English Channel.

The isotope measurements (Fig. 1) show an estimated temperature range from 15.5 to 21.5 °C, with lower temperatures in the *G. semivera* subzone and higher in the succeeding *G. praescitula* subzone. Elsewhere *Globorotalia praescitula* first appears before the *G. trilobus* zone¹⁰ so that its local appearance in the English Channel may have been controlled by temperature.

Figure 1 shows species diversity of planktonic foraminifera¹ as well as temperature. In today's ocean there is a definite relationship between species diversity and temperature⁴ although other factors contribute to diversity patterns. Previous work has shown a broad correlation between species diversity and oxygen isotope palaeotemperature in the New Zealand Cainozoic^{11,12} but the comparison was done on the basis of total diversity of a section rather than on the diversity in individual samples. Figure 1 shows a remarkable detailed relationship between diversity and isotopic palaeotemperature among the individual samples studied.

Over geological time, species diversity is obviously likely to be controlled by evolutionary processes¹², as well as by changes in the competitive balance between different faunal groups. However, in the present situation we are studying changes within a single zone, and perhaps only over an interval of about a million years, so that neither factor is likely to contribute to varying species diversity. In this circumstance it is perhaps not surprising that the relationship between diversity and temperature is so clear. It is, therefore, of considerable interest because it opens up the possibility of estimating short-term changes in temperature in situations where the physical method of estimating palaeotemperature cannot be applied.

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D. GRAHAM JENKINS

Department of Earth Sciences,
Open University,
Milton Keynes, UK

NICHOLAS SHACKLETON

Sub-Department of Quaternary Research,
University of Cambridge,
Cambridge, UK

Received 13 October 1978; accepted 22 January 1979.

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Two separate photoreceptors control hypocotyl growth in green seedlings

PHOTOCNTROL of elongation growth has been extensively studied in dark-grown (etiolated) seedlings for some time. However, only relatively recently has detailed attention been given to green (de-etiolated) plants. One reason for this is that the photoreceptor, phytochrome, can easily be detected spectrophotometrically in non-green tissues but not in tissue containing chlorophyll. There is agreement that the photoreceptor operates in its low-energy mode (low irradiances or fluence rates, with red/far-red reversibility) in green seedlings as well as in dark-grown, non-green seedlings. In its high-energy

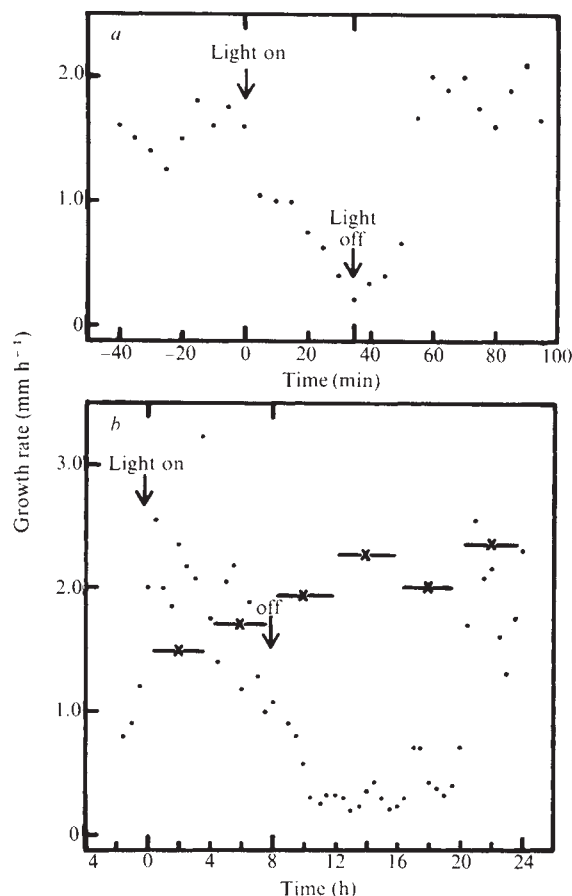


Fig. 1 Growth kinetics in response to illumination and darkness. *a*, Blue-light inhibition and recovery: blue light was provided by an opposed pair of collimators (to prevent phototropism) focused on the 5-mm region of the hypocotyl measured ~10 mm from the cotyledonary node. The light source consisted of 150-W quartz-halogen bulbs in projectors, and the light from each was passed through a layer of Cinemoid (deep blue no. 20) and 30 mm of 2.5% cupric chloride (to eliminate far-red) and an additional lens for focusing. This combination gives broad-band light, from 400–510 nm (peak 440 nm; half-band width ~45 nm). (Fluence rate as in Table 2.) *b*, Red light inhibition and recovery. Diffuse red light was provided by light from 20-W Gro-lux fluorescent tubes filtered through a layer of Cinemoid (ruby red no. 14). This combination gives broad-band red light from 600–710 nm (peak 660 nm; half-band width ~20 nm). (Fluence rate as in Table 2—saturation is given by ~1 W m⁻².) Crosses mark the growth rate of dark control seedlings (having had a similar pretreatment). Each cross represents the mean growth rate for a 4-h period, the limits of which are shown by the horizontal bar (measurements by ruler, $n = 12$).

mode (high irradiances and time dependency), exemplified by the action of prolonged irradiation with far-red, phytochrome might act only in etiolated seedlings¹⁻³. Photoregulation of stem extension in etiolated and de-etiolated seedlings is also achieved by blue light (400–500 nm), but it is still not clear if a photoreceptor other than phytochrome is responsible for this effect. Kinetic studies on etiolated seedlings suggest the operation of a separate blue-responsive photosystem². Notwithstanding this, it has recently been suggested that in green seedlings blue-light control of hypocotyl extension is due solely to absorption by phytochrome⁴. This claim, together with the evidence that changes in phytochrome photoequilibria can exert profound effects upon stem extension, may lead one to believe that this is the major photoreceptor controlling elongation growth in green plants. We report here, however, work that strongly indicates that two separate pigment systems control hypocotyl extension in green seedlings—phytochrome and a separate blue-light

receptor. Both systems produce the same response—inhibition—but we have found it possible to separate their effects by examining the times at which the response commences.

We have examined in detail the kinetics of the growth responses to light in seedlings of *Cucumis sativus* L which, 3.75 days after germination and growth in darkness (when hypocotyls were 30–40 mm long), were de-etiolated by exposure to white, fluorescent light ('daylight') for 8 h, followed by a 16-h dark period, that is, one 'short-day' pretreatment. At the end of the dark period plants were subjected to the experimental treatments. All plants were grown, and all experiments performed at 25°C. This de-etiolation treatment completely opens the hypocotyl hook, and causes greening and enlargement of the cotyledons. The seedlings are not morphologically identical to, say, the seedlings de-etiolated by 30 h of white light as used by Black and Shuttleworth¹, but they do have the same physiological characteristics as these, that is, phytochrome control of hypocotyl extension by the cotyledons, and loss of the far-red component of the high irradiance reaction¹. Responses with identical timings to those reported below have been obtained with seedlings de-etiolated for 30 h with white light. Returning such seedlings to darkness for several hours gives the same high growth rates as shown by the presently-used material. High growth rates are required for a detailed study of growth kinetics, especially the onset of photoinhibition. Hypocotyl extension in response to light and darkness was monitored using a linear-displacement transducer (ND2, Sangamo Weston Controls Ltd.) in which the counterbalanced, moveable armature was placed at the apex of the plant, between the two cotyledons. After a period of equilibration, extension was recorded continuously and transformed to growth rates (mm h⁻¹) computed at intervals over the growth period. (The few points before 'light on' in Fig. 1*b* illustrate the pre-equilibration rise in growth rate.) A good deal of variability in growth rates is encountered in measurements by transducers. We have found that although the absolute growth rate may vary considerably the timing of the photoresponses is remarkably constant (Table 1). The initial high growth rates of our seedlings allowed us to determine this timing with great definition. Results obtained with conventional measuring techniques are fully consistent with the data reported here which are concerned with the 'ultrastructure' of growth, as it were, obtainable only by means of a highly sensitive device such as a transducer.

Kinetics of light-induced inhibition and of the subsequent recovery in darkness clearly depend upon the spectral region (Fig. 1). The growth rate rapidly falls after about 5 min exposure to blue light, and inhibition reaches a maximum (that is a minimum growth rate) some 35 min later (Fig. 1*a*). On the other hand, red light-induced inhibition starts only 5–6 h after exposure to red light (Fig. 1*b*) and reaches a maximum at 10 h. That this is phytochrome-controlled inhibition is shown by the effectiveness of short periods of red light (10 min every hour), each period being far-red reversible. Thus, to produce photoinhibition several 'inputs' of P_r are required. In practice this is most easily achieved by continuous exposure to red light. The effects of continuous and intermittent red light are, however,

Table 1 Timing of photoresponses controlling hypocotyl extension

Treatment	Time to initial response (mean ± s.e.m.)	
	Inhibition	Recovery
White fluorescent light	4.25 ± 0.29 min ($n = 28$)	Short:
		18.9 ± 1.8 min ($n = 9$)
Red light	5.8 ± 0.31 h ($n = 21$)	Long:
		10.45 ± 0.49 h ($n = 19$)
Blue light	5.0 ± 0.55 min ($n = 9$)	10.0 ± 0.5 h ($n = 13$)
		18.9 ± 3.8 min ($n = 9$)

'Short' indicates those white light treatments where the irradiation was given for less than the time for the appearance of phytochrome-controlled inhibition (that is, <3 h). 'Long' refers to those light periods during which phytochrome-controlled inhibition sets in (that is, >8 h).

very close. Moreover, the input of P_{fr} over periods of 1 h and longer produce inhibition, always commencing about 6 h after the beginning of irradiation. We stress that blue light acts directly on the hypocotyl as already reported^{1,6,7}. We observe the same inhibition kinetics in both blue and white light (see below) whether or not the cotyledons are covered. In red light also, covering the cotyledons does not alter the time of onset of inhibition but the 'depth' of inhibition and the timing of recovery are both affected. These latter effects reflect the contribution to photoinhibition made by the cotyledons^{1,6}; the onset of inhibition is presumably due to the action of red light directly upon the hypocotyl^{1,6,7}.

At some time after the end of illumination plants begin to recover from photoinhibition and show a gradually increasing growth rate until a maximum 'dark' rate is obtained. The kinetics of these changes again depend on the spectral region used for previous illumination (Fig. 1a, b). In this and most experiments, recovery from blue light starts slowly, as soon as 5 min after the end of irradiation, with the major increase between 15 and 20 min of darkness, and is complete soon after; but in some experiments the start of recovery was delayed for up to 15–20 min. Red-light inhibition, induced through phytochrome, is much more persistent; about 8 h of darkness are required before the growth rate begins to recover. Thus, blue light inhibits virtually only during irradiation whereas phytochrome-mediated, red-light inhibition has a delayed onset and recovery. Such widely disparate kinetics are difficult to explain on the basis of one pigment system and we conclude, therefore, that hypocotyl extension in de-etiolated *Cucumis* is controlled by two photoreceptors, one being phytochrome and the other an as yet incompletely identified system, specific to blue light^{8,9}. That blue light does not operate through phytochrome in our experimental conditions is further substantiated by the inability of background far-red to alter either the extent or the timing of blue-light inhibition, even though $\phi(P_{fr}:P_{total})$ is reduced from 0.44 to 0.14 (Table 2). There is clearly no correlation between ϕ and the kinetics of photoinhibition.

What of the action of white light? White fluorescent light contains both blue and red wavebands and it therefore activates the specific blue light-sensitive system as well as phytochrome. Onset of inhibition is accordingly rapid (due to blue light) and recovery in darkness after exposure to several hours of white light is slow to develop, presumably because of the long-lasting inhibition caused by phytochrome. It is possible, however, by adjusting the length of the white-light period, to separate the timings of both sets of inhibition and recovery in a single experiment (Fig. 2). Periods of relatively short duration, for example 2–3 h as used in this experiment, produce the rapid inhibition typical of blue light. The characteristic phytochrome-controlled inhibition (red light) does not commence during the

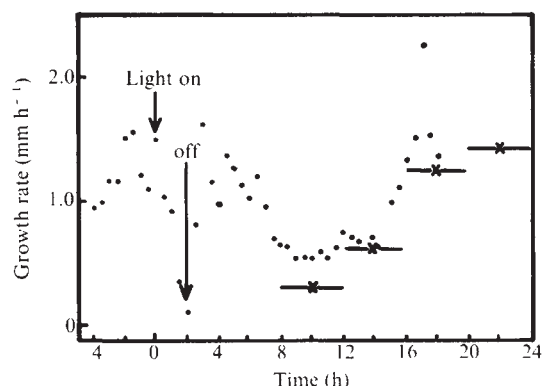


Fig. 2 Effect of a brief white light irradiation. The white light source consisted of 20-W 'daylight' tubes. (Fluence rate as in Table 2.) Crosses mark the mean growth rates calculated every 4 h (shown by the horizontal bar) of similarly treated seedlings, measured by ruler ($n = 12$). As in Fig. 1b these indicate that the seedlings merely recover their growth rate, showing no entrained rhythm.

Table 2 Effect of $P_{fr}:P_{total}$ on the kinetics of photoinhibition

Light source	Fluence rate	$\phi(P_{fr}:P_{total})^*$	Approximate time to start of inhibition (min)
White fluorescent	34 W m ⁻²	0.75	5
Red	8 W m ⁻²	0.75	300
Blue	24 W m ⁻²	0.44	5
Blue + far-red	24 + 6.5 W m ⁻²	0.14	5

*Measured in 15-mm long hypocotyl sections (including hooks) of 3.5-d-old dark-grown seedlings of *Cucumis sativus* L. using a Perkin Elmer model 156 double wavelength spectrophotometer.

time that the plants are actually exposed to white light, as it requires 5 h for its development to begin (Fig. 1b). Twenty minutes after return to darkness a recovery of growth rate is apparent but ~2 h later this is followed by a second, transient inhibition which occurs in darkness. This comparatively short exposure to white light therefore produces dual kinetics and, we suggest, succeeds in revealing the temporal separation of blue light and phytochrome actions.

The two components of inhibition seem difficult to accommodate in a hypothesis which assigns a role to one pigment only—phytochrome—and appear to us to be best explained as the operation of two separate photosystems, each having different kinetics, in the following manner. The rapid inhibition is due to the blue light effect from which seedlings begin to recover at the end of the light period. The red component of white fluorescent light produces high levels of P_{fr} whose inhibitory action, beginning 5 h after the start of illumination, makes its appearance about 3 h after the plant has returned to darkness. The final recovery of growth rate is the delayed release from phytochrome-induced inhibition. In longer white light periods the actions of the two photosystems become superimposed. Inhibition quickly sets in but the rapid recovery from blue light is obliterated by the long-lasting, red light-induced inhibition, and the observed recovery upon return to darkness thus has only the kinetics which follow phytochrome photoinhibition.

The amount of inhibition caused by blue light depends strongly upon the irradiance, though the timing of the start of inhibition does not. Here we have used high irradiances (fluence rates) to produce substantial and clearly recognisable reductions in growth rate. Pronounced irradiance dependency with regard to the magnitude of inhibition is not found in the case of red light and saturation is achieved by relatively low fluence rates (about 1 W m⁻²). It therefore seems likely that in nature the dependency of hypocotyl and stem extension upon the fluence rate of white light is a response to the blue component. We further suggest that the blue-light system detects fluence rate differences while phytochrome is concerned more with light quality, particularly red/far-red ratios, a point which has been raised by other workers¹⁰. Morgan and Smith¹¹ reported rapid effects of white light on stem extension in de-etiolated *Chenopodium*. The similarity of some of their kinetics to those reported here suggest that the responses of *Chenopodium* to irradiance changes may be attributable to the blue light receptor.

Finally, we should add that the kinetics discussed above account for various growth patterns shown by seedlings held under different daily light regimes. Details of these will be reported elsewhere.

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VICTOR GABA
MICHAEL BLACK

Department of Biology,
Queen Elizabeth College,
University of London,
Campden Hill Road, London W8, UK

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In vitro duplication of the primary light-induced charge separation in purple photosynthetic bacteria

THE primary light-induced electron transfer events of both green plant and bacterial photosynthesis result in very rapid charge separation which subsequently generates a chemical potential within the organism. There is much information concerning the primary events of bacterial photosynthesis but for green plants the details of these processes are just beginning to become available. Strong evidence based mainly on electron spin resonance and electron-nuclear double resonance studies of oxidised bacterial reaction centres favours a dimeric bacteriochlorophyll *a* (BChl *a*) structure for the primary photochemical electron donor¹. The dimeric BChl *a* species absorbs light at 865 nm in reaction centres from *Rhodospseudomonas sphaeroides* yielding an excited state which rapidly transfers an electron to a molecule of bacteriopheophytin *a* (BPh *a*) in <10 ps (refs 2–4). Within about 150 ps the electron is transferred to a quinone molecule which in turn transfers an electron to a secondary quinone within a few microseconds^{5,6}. In reaction centres for which the quinones have been either extracted or

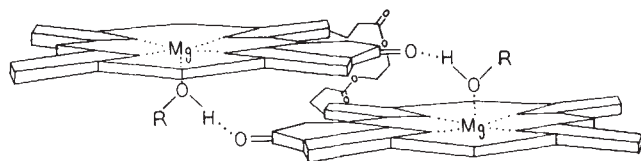


Fig. 1 PChl *a* dimer in its folded C_2 symmetric conformation.

chemically reduced before light excitation, BPh[−] back transfers an electron to (BChl *a*)₂⁺ in 10–20 ns (ref. 7). Thus, the *in vivo* geometry of the reaction centre is such that the reverse electron transfer is $5\text{--}10 \times 10^3$ times slower than the forward reaction. We now report the first *in vitro* model that duplicates both the rapid light-induced charge transfer and the slow back reaction of the reaction centre.

Covalently linked dimers of BChl *a*, chlorophyll *a* (Chl *a*) and pyrochlorophyll *a* (PChl *a*) have been prepared which mimic the spectroscopic and redox properties of reaction centre chlorophylls^{8–10}. The two chlorophylls are joined in each case at their propionic acid side chains through an ethylene glycol diester

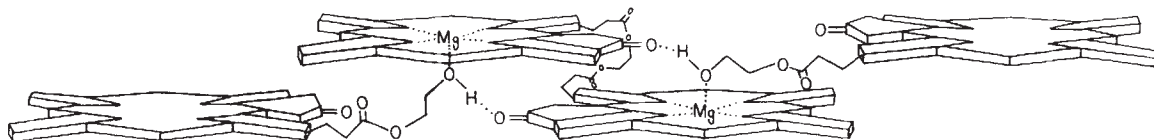


Fig. 2 Proposed structure of the (PChl *a*)₂-(PPhide *a*-OH)₂ reaction centre model.

linkage. The orientation of the chlorophyll macrocycles with respect to one another, and consequently their electronic properties, are highly solvent dependent. Treating solutions of these covalent dimers in dry non-nucleophilic solvents (such as benzene, toluene, CCl₄) with 10–100-fold molar excesses of a hydrogen-bonding nucleophile (such as water, primary alcohols, primary thiols) results in their folding into the C_2 symmetric conformation shown in Fig. 1. This structural assignment is based primarily on ¹H NMR spectral evidence^{8,10}. The folding is accompanied by shifts of both the longest wavelength absorption in their electronic transition spectra and their fluorescence maxima to longer wavelengths.

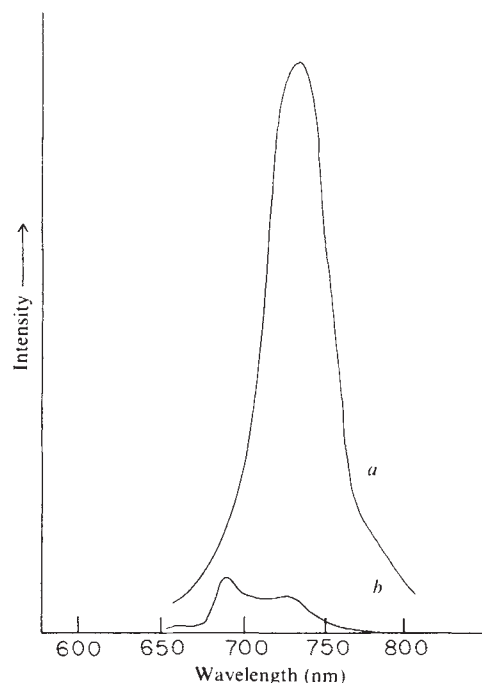


Fig. 3 *a*, Fluorescence spectrum of 0.2 mM PChl *a* dimer in toluene containing 20 mM ethanol. Excitation at 530 nm. *b*, Fluorescence spectrum of 0.2 mM PChl *a* dimer in toluene containing 0.8 mM PPhide *a*-OH. Excitation at 530 nm.

We used the PChl *a* dimer to produce a functional model reaction centre because of its inherent stability towards photo-degradation¹¹. The PChl *a* dimer was prepared by a method described elsewhere⁸. Folding this dimer into the conformation shown in Fig. 1 results in a shift of its absorption maximum from 663 nm to 694 nm, whereas its fluorescence maximum shifts from 670 nm to 735 nm. To position pheophytin *a*, which serves as the electron acceptor, close to the dimer, we have used the strategy of replacing the hydrogen-bonded short-chain alcohol which secures the conformation of the dimer shown in Fig. 1 with a primary alcohol derivative of pheophytin *a*. The pyropheophorbide *a* ethylene glycol monoester (PPhide *a*-OH), a precursor of the PChl *a* dimer, proves to be ideal for this.

When dry 0.2 mM solutions of PChl *a* dimer in toluene are treated with as little as a fourfold molar excess of PPhide *a*-OH at room temperature or a stoichiometric twofold molar excess at

0 °C, the dimer readily folds into the structure shown in Fig. 2 (^1H and ^{13}C NMR structural evidence, M.R.W., unpublished). This folding is accompanied by a shift of the dimer electronic absorption maximum to 694 nm. More importantly, the 735 nm fluorescence of the dimer is almost completely quenched (Fig. 3). If a similar experiment at the same concentrations is carried out with PChl *a* dimer folded with ethanol while unbound pheophytin *a* is present in the solution, the fluorescence quantum yield of the dimer actually increases by about 10% over that seen in the absence of pheophytin *a*. This effect may be attributed to excitation energy transfer from excess pheophytin *a* in solution to the folded dimer which acts as an energy trap.

To determine the mechanism and kinetics of the quenching of the PChl *a* dimer fluorescence by PPhide *a*-OH in our model reaction centre, we have determined the excited state difference spectrum of the model as a function of time on the picosecond time scale. The experimental apparatus and techniques used to obtain the data have been described elsewhere¹². Fig. 4 shows the difference in optical density between the excited state and the ground state following a 6-ps flash at 530 nm.

The rise time for all ΔA changes is <6 ps. These changes persist for 10–20 ns with the subsequent return of the ground state absorption spectrum. The spectrum in Fig. 4 displays a positive ΔA change at 800 nm and strong bleaching at 660 nm.

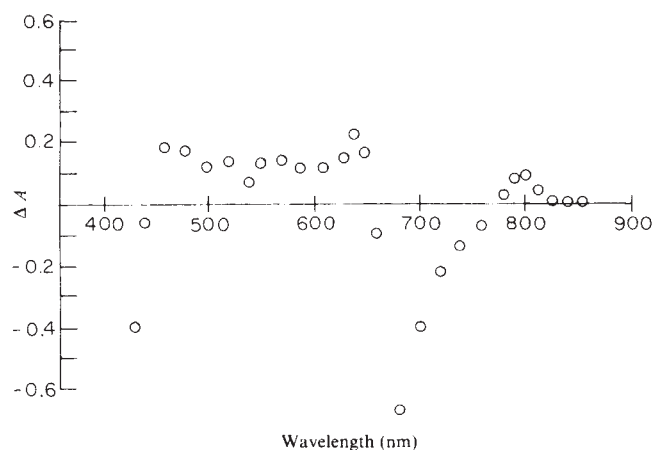


Fig. 4 Initial absorption changes following 6 ps excitation with 530 nm light of 0.2 mM PChl *a* dimer and 0.8 mM PPhide *a*-OH in toluene at 20 °C.

Positive ΔA changes dominate the 440–630-nm region of the spectrum. The 800-nm feature in the spectrum is characteristic of a Chl *a* cation radical¹³. The one electron oxidation product of the PChl *a* dimer folded with ethanol displays a broad electronic absorption near 800 nm. Similarly, P₇₀₀ in green plants undergoes light-induced oxidation to yield a species which absorbs light near 800 nm (ref. 14). The strong bleaching at 660 nm and the smaller bleachings at 500 nm and at 530 nm can be ascribed to the disappearance of ground state PPhide *a*-OH. One electron reduction of pheophytin *a* results in very similar spectral changes¹⁵. Thus, the overall spectrum in Fig. 4 may be adequately described as a superposition of the PChl *a* dimer cation radical spectrum and the PPhide *a*-OH anion radical spectrum. Whether the complex exists as a radical pair or as a charge transfer state has not yet been ascertained. The overall spectral features strongly parallel the type of spectral changes observed following flash excitation of bacterial reaction centres even though the wavelengths of the ΔA changes are different for the *a*-series plant chlorophylls^{2–4}. Based on the fluorescence quenching data and assuming reasonable values for the oscillator strength of the 800-nm transition in (PChl *a*)₂⁺, the quantum efficiency of electron transfer in our model approaches

unity. Thus, our model reaction centre channels most of the light energy it absorbs into an electron transfer process which displays the same asymmetric kinetics as observed *in vivo*.

It is clear from our experiments that positioning the PPhide *a*-OH electron acceptor close to the PChl *a* dimer electron donor is important for efficient light-induced electron transfer. However, the kinetics of both the forward and reverse transfers depend critically on the energies of the states involved. Excitation of the PChl *a* dimer into its first excited singlet state raises its energy to ~ 1.8 eV above the ground state. Charge separation is then an energetically 'downhill' process with the resultant ion pair state ~ 1.5 eV above the ground state (estimated from the difference between the one electron oxidation potential of the folded PChl *a* dimer, 0.50 V compared with SCE, and the one electron reduction potential of PPhide *a*-OH, -1.02 V compared with SCE, in methylene chloride). The ~ 0.3 -eV energy difference between the singlet and ion pair states is sufficient for a rapid forward transfer rate, but results in the reverse electron transfer being an activated process. Thus, the rate of an electron transfer leading back to the first excited singlet state should be substantially slower than the charge separation rate.

The only reasonable pathway remaining for the ion pair state is to cross over to a highly vibrationally excited state of the ground electronic state. As $\Delta G < 0$, ~ -1.5 eV, the Franck-Condon factors for this process are small and thus the rate of return to the ground state is relatively slow.

Because the PPhide *a*-OH molecules may exhibit some conformational freedom due to rotation about the single bonds of their alkyl chains, the role of these changes following excitation and their effect on the reverse electron transfer rate need to be further explored. In conclusion, we have successfully modelled the primary light-induced charge separation chemistry in reaction centres of purple photosynthetic bacteria by using *in vitro* reaction centre models based on chlorophyll derivatives.

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MICHAEL J. PELLIN
KENNETH J. KAUFMANN*

Department of Chemistry,
University of Illinois,
Urbana, Illinois 61801

MICHAEL R. WASIELEWSKI*

Chemistry Division,
Argonne National Laboratory,
Argonne, Illinois 60439

Received 17 October 1978; accepted 4 January 1979.

* To whom correspondence should be addressed.

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Antigen specific T-cell helper factor cross reacts idiotypically with antibodies of the same specificity

THE recognition system of bone marrow-derived (B) cells and their products is based on the interaction of the active site of immunoglobulins with the specific antigenic determinants. However, the molecular nature of the recognition component of thymus-derived (T) lymphocytes is not yet established. Studies on antigen specific T-cell helper or suppressor factors¹⁻⁵ and isolation of the antigen receptor directly from T cells, or from serum^{6,7} are examples of approaches to elucidate this problem. The synthetic polypeptide poly(Tyr,Glu)-poly(DLAla)--poly(Lys) [abbreviated (T,G)-A--L] has been extensively used in studies on the molecular basis of antigenicity⁸ and genetic regulation of immune responsiveness⁹⁻¹¹. Studies on the antibody response to (T, G)-A--L have revealed the involvement of several cell types and a T-cell replacing soluble factor which are antigen specific and genetically controlled¹²⁻¹⁴. An important feature of the T-cell factors is their antigen specificity. The specificity of a T-cell factor produced to (T, G)-A--L was demonstrated by the removal of its activity on specific antigen-Sepharose columns¹⁵ and also by demonstrating that the *in vivo* helper effect was specific to the antigen used in the T cell education^{1,16}. It is very likely that the specific T-cell factor represents the molecular recognition component of T cells—the T-cell receptor. If this is the case, the molecular identification and characterisation of the T-cell factor may answer one of the most controversial questions in immunology. It has been suggested that antigen-specific molecules of B and T cells share idiotypic determinants^{17,18}. It was, therefore, of interest to find out whether the (T, G)-A--L specific factor will share idiotypic determinants with antibodies of the same specificity. The results of the present study demonstrate that B-cell products (antibodies) and T-cell products (factors) possess cross-reacting idiotypic determinants. This strongly suggests the presence of similar variable regions in both antigen binding entities, which are probably coded by the same variable region genes. Cross-reactive idiotypic determinants are also present on (T, G)-A--L specific factors obtained from low responder C3H/DiSn T cells confirming previous reports on the activity of these factors.

The successful production of anti-idiotypic antisera in guinea pigs to C3H.SW anti-(T, G)-A--L antibodies has been recently reported¹⁹. Idiotypic determinants recognised by these sera were present in anti-(T, G)-A--L antibodies from different individuals of C3H.SW mice. A linkage between the expression of the cross-reactive idiotypes and heavy chain constant region allotypes was demonstrated¹⁹. This characterised anti-idiotypic serum was used in the present study to look for the presence of idiotypic determinants on the (T, G)-A--L specific T-cell factor. Indeed, the activity of a (T, G)-A--L specific factor was removed after passage through an immunoabsorbent of Sepharose-coupled anti-idiotypic serum.

The (T, G)-A--L specific T-cell factors were produced by 'educated' T cells of C3H.SW, CWB and C3H/DiSn inbred mouse strains as described elsewhere²⁰. The helper activity of the factors was assayed by transferring them into irradiated mice together with high responder (C3H.SW) bone marrow cells and (T, G)-A--L²⁰. The immune response was followed by determining the anti-(T, G)-A--L haemagglutination titres in the sera of the recipients²⁰. The anti-idiotypic serum prepared in guinea pigs against C3H.SW anti-(T, G)-A--L antibodies was the same as previously reported¹⁹. The IgG fraction (10 mg) of the anti-idiotypic serum was coupled to 1 g of Sepharose²¹.

As shown in Table 1, a factor produced by C3H.SW (high responder) educated T cells replaced the helper function of thymocytes in eliciting an antibody response to (T, G)-A--L. The activity of this factor was completely removed by passage through the anti-idiotypic-Sepharose immunoabsorbent,

whereas no effect on the activity of the factor was observed when normal guinea pig IgG or guinea pig anti-mouse IgG were used as immunoabsorbents.

C3H/DiSn (H-2^k) low responder mice are capable of producing a (T, G)-A--L specific factor which cooperates with high responder (H-2^b) B cells for the production of antibodies (see Table 1 and ref. 20). The factor produced by C3H/DiSn mice was removed by the anti-idiotypic antibodies. As with the (T, G)-A--L specific factor from C3H.SW mice, the activity of the factor from C3H/DiSn mice was completely retained after passage through guinea pig anti-mouse IgG immunoabsorbent.

Table 1 Effect of anti-idiotypic antibodies on the helper activity of (T, G)-A--L specific T-cell factors

C3H.SW bone marrow cells and (T, G)-A--L transferred with:	No. of animals	No. of experiments	log ₂ haemagglutination titres
No addition	31	4	0.77 ± 0.2
10 ⁸ C3H.SW thymocytes	19	4	4.21 ± 0.6
C3H.SW factor	30	4	4.37 ± 0.03
C3H.SW factor passed on an anti-idiotypic-Sepharose column	30	4	1.03 ± 0.21
C3H.SW factor passed on a normal guinea pig-Sepharose column	19	3	4.74 ± 0.35
C3H.SW factor passed on a guinea pig anti-mouse IgG-Sepharose column	6	1	3.7 ± 0.34
C3H/DiSn factor	11	2	4.0 ± 0.22
C3H/DiSn factor passed on an anti-idiotypic-Sepharose column	11	2	1.09 ± 0.39
C3H/DiSn factor passed on a guinea pig anti-mouse IgG-Sepharose column	12	2	3.75 ± 0.18
CWB factor	6	1	4.0 ± 0.36
CWB factor passed on an anti-idiotypic-Sepharose column	4	1	4.0 ± 0

C3H.SW recipient mice were irradiated (850 R) and reconstituted with 2×10^7 syngeneic bone marrow cells, 10 µg of (T, G)-A--L and either thymocytes or (T, G)-A--L specific T-cell factors produced by one spleen equivalent of educated T cells. Mice were bled 12 d after transfer and their sera assayed by passive haemagglutination with (T, G)-A--L coated sheep red blood cells. Values are means ± s.e.m.

CWB (H-2^b, Ig-1^b) mice are high responders to (T, G)-A--L. They are congenic with C3H.SW (H-2^b, Ig-1^a) mice and differ only in their heavy chain allotypic markers. Anti-(T, G)-A--L antibodies of CWB mice do not share idiotypic determinants with antibodies of C3H.SW mice¹⁹. It was, therefore, of interest to find out whether a factor of CWB mice would react with the anti-idiotypic antibodies. As demonstrated in Table 1, the CWB factor was not affected by the anti-idiotypic antibodies against C3H.SW anti-(T, G)-A--L.

As shown for antibodies, the ability to produce a (T, G)-A--L specific factor which cross reacts with C3H.SW anti-(T, G)-A--L antibodies is linked to the Ig-1 allele of the mouse strain.

Note that the anti-idiotypic serum bound only 30% of the idiotypes (C3H.SW anti-(T, G)-A--L antibodies)¹⁹ whereas in this study we have demonstrated that the activity of the factor was completely removed by the anti-idiotypic immunoabsorbent. As the activity of the factor is lost on dilution (at fourfold or higher dilutions, data not shown) it is possible that most but not all of the factor present was removed by the anti-idiotypic. It is, of course, possible that the particular type of the gene product

that is expressed in the anti-(T, G)-A--L antibodies to the extent of 30% and bears the cross-reactive idiotype determinants, is expressed to a higher extent in the population of the factor molecules of the same specificity. This, however, does not exclude the possibility that T- and B-cell products have potentially the same repertoire but that regulation processes determine a different expression of a particular type in one or the other type of recognition entities. Indeed, it is clear that antibodies of different classes with the same specificity share the same V region repertoire but that antibodies of the IgG class reach a higher affinity upon maturation than those of the IgM class²². It has also been shown recently in another system that the T-cell receptor is restricted to only part of the idiotypes expressed by antibodies²³.

In contrast to our findings in mice, it has been reported that an antigen-specific T-cell factor produced in rabbits did not show the presence of V_H markers²⁴ namely a locus allotypes, whereas a receptor directly isolated from rabbit T cells did show the presence of V_H-related allotypic determinants²⁵.

It is noteworthy that the antigen-specific T-cell helper factors have been shown to possess Ia determinants^{1,2,26}. Thus, these factors are coded by two unlinked genes. A similar conclusion has been drawn from genetic studies with an anti-idiotypic serum to a T-cell receptor²⁷. The molecular basis of the combination between I region gene products and immunoglobulin variable region gene products is not yet understood. Preliminary experiments in our laboratory suggest that the two determinants are part of one entity since combination of effluents passed separately on anti-Ia and anti-idiotypic immunoadsorbents did not result in an active helper factor.

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EDNA MOZES
JOSEPH HAIMOVICH*

Department of Chemical Immunology,
The Weizmann Institute of Science,
Rehovot, Israel

Received 30 October 1978; accepted 3 January 1979.

*Present address: Department of Human Microbiology, Sackler School of Medicine, Tel-Aviv University, Tel-Aviv, Israel.

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Is synaptic facilitation caused by presynaptic spike broadening?

FACILITATION is a characteristic of many synapses whereby the efficacy of synaptic transmission increases following presynaptic activity. This form of neuronal plasticity has been implicated in behavioural modifications such as habituation¹ and learning². Facilitation has been shown to be due to a presynaptic increase in transmitter release³, but its biophysical mechanism remains obscure. Although it is often suggested that facilitation is due to the action of a residual increase in intracellular free calcium⁴, an alternative hypothesis, that facilitation may be caused by action potential prolongation, has recently been proposed⁵⁻⁷. In molluscan neural somata, spike broadening occurs in a train of impulses, due to non-inactivating calcium channels and cumulatively inactivating potassium channels⁷. This might result in an increased calcium influx associated with prolonged spikes⁸, which would lead immediately to facilitated transmitter release⁹. We show here that at crustacean neuromuscular junctions, spike broadening is not primarily responsible for facilitation.

In previous work on neuromuscular junctions, no presynaptic spike broadening was observed, and facilitation could be evoked by repeated terminal depolarisation^{10,11}. However, the extracellular recording method used is particularly insensitive to changes in spike duration¹², and a cumulative potassium inactivation would invalidate the interpretation¹³ of the effect of extracellular stimulation on presynaptic nerve polarisation. Moreover, the effect of caesium on neuromuscular transmission is consistent with the spike broadening facilitation hypothesis. Caesium prolongs nerve terminal potentials (n.t.ps) (probably by blocking potassium channels), increases the number of quanta released by an impulse, and reduces facilitation at crab neuromuscular junctions^{14,15}. Similarly, in molluscan somata, when the inactivating outward current is blocked pharmacologically, the spikes are all broadened, but progressive spike prolongation in a train is prevented.

An alternative explanation for the caesium effect¹⁰ is that the longer n.t.ps may simply be releasing all the transmitter immediately available for release, so that the transmitter release is saturated. Then, whatever processes are responsible for facilitation may still be operable in caesium, but are not expressed, as facilitation of the already saturated release of transmitter is impossible.

These alternatives may be distinguished by soaking neuromuscular junctions in a caesium-substituted potassium-free saline, and then reducing calcium and raising magnesium

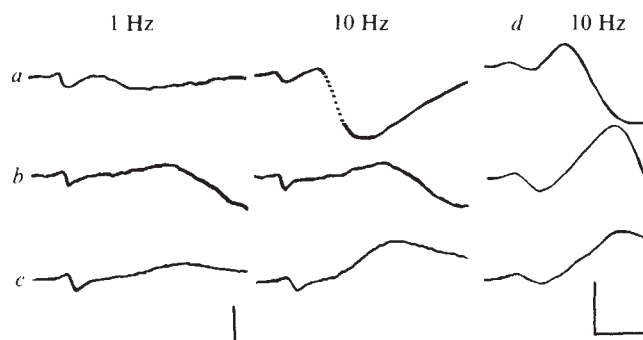


Fig. 1 Extracellular recordings of nerve potentials and synaptic currents from synaptic sites. *a-c* Are from three separate preparations and show responses at 1 and 10 Hz in normal saline (*a*), 5.4 mM potassium replaced by caesium with normal calcium and magnesium (*b*), and 5.4 mM caesium with 1 mM calcium and 15 mM magnesium (*c*). *d*, Responses from a single preparation stimulated at 10 Hz are shown in the three experimental solutions. All traces are computer-averaged responses to 64 stimuli. Calibration bars: 0.1 mV, 2 ms.

Table 1 Effects of caesium or 4-aminopyridine in normal and low-calcium media on synaptic transmission and facilitation

Experiment	Solution	e.j.p. amplitude (mV) at frequency of		
		1 Hz	10 Hz	Facilitation
1	Control	0.19	1.08	4.68
	Cs	0.95	3.37	2.54
	Cs low-Ca	0.36	1.67	3.64
2	Control	0.15	1.37	8.13
	Cs	15.58	37.97	1.44
	Cs low-Ca	0.12	0.95	6.92
3	Control	1.50	8.89	5.93
	Cs	4.75	15.50	3.26
	Cs low-Ca	0.38	4.00	10.53
4	Control	0.64	2.44	2.81
	Cs	2.33	4.48	0.92
	Cs low-Ca	0.25	0.77	2.08
5	Control	0.60	2.30	2.83
	Cs	5.00	12.50	1.50
	Cs low-Ca	1.00	4.50	3.50
6	Control	0.16	0.78	3.84
	Cs	1.80	6.00	2.33
	Cs low-Ca	0.60	2.80	3.67
7	Control	0.19	0.55	4.50
	AP	3.55	5.00	0.49
	AP low-Ca	0.30	0.85	1.83
8	Control	0.10	0.44	3.40
	AP	2.10	5.00	1.38
	AP low-Ca	0.08	0.30	2.75
9	Control	0.23	0.45	0.96
	AP	3.36	5.73	0.71
	AP low-Ca	0.04	0.12	2.00
10	Control	0.19	0.28	0.47
	AP	4.44	5.48	0.32
	AP low-Ca	0.07	0.18	1.57
11	Control	0.10	0.44	3.40
	AP	1.01	2.34	1.32
	AP low-Ca	0.06	0.20	2.33
12	Control	0.07	0.35	4.00
	AP	1.18	2.50	1.12
	AP low-Ca	0.12	0.80	5.67

'Control', normal saline (as in ref. 3, except that 10 mM HEPES buffer is used to hold pH to 7.3); 'Cs', 5.4 mM caesium substituted for potassium; 'AP', saline plus 25 μ M 4-aminopyridine; 'Cs low-Ca', 5.4 mM caesium substituted for potassium, calcium reduced from normal (13.5 mM) to 1 mM, and magnesium raised from normal (2.5 mM) to 15 mM; 'AP low-Ca', saline with 25 μ M 4-aminopyridine, 1 mM calcium and 15 mM magnesium. In the last two solutions, the calcium and magnesium concentrations were buffered with EDTA¹⁶ and the calculated calcium concentration was confirmed with an Orion ion-selective electrode. Excitatory junctional potential (e.j.p.) amplitudes were corrected for the nonlinear relation between transmitter release and e.j.p. amplitude, using Martin's revised correction procedure²², which is applicable to crustacean muscles because their multi-terminal innervation and short electrotonic length make them nearly isopotential²³. The membrane time constant was estimated from the e.j.p. falling phase and the synaptic current time course was derived from the e.j.p. time course²⁴ or measured from the extracellularly recorded synaptic current. Caesium and low calcium had little effect on the synaptic current time course, so errors in estimating transmitter release due to changes in release synchrony were averted^{25,26}. Each e.j.p. amplitude given is the average of at least 10 measurements. The standard errors were lower than 11% in each case (typically 5%) and are not shown. Facilitation is expressed as the fractional increase in transmission at 10 Hz compared with that at 1 Hz. A facilitation index of unity signifies a doubling in transmitter release.

until transmitter release is restored to near the control level in normal saline. As either calcium reduction or blockade of potassium currents prevents spike broadening in molluscan somata, both treatments combined ought to be doubly effective in preventing facilitation due to spike broadening. In contrast, if spike broadening does not normally occur at crustacean nerve terminals and has nothing to do with facilitation, reducing calcium and elevating magnesium should return transmitter release to a non-saturated level and facilitation should reappear. It was shown previously¹⁶ that changes in calcium concentration alone do not affect facilitation.

Table 1 shows the results of six experiments in which we recorded intracellularly from crayfish (*Procambarus clarkii*) claw opener muscles while stimulating the opener motor neurone, using procedures described elsewhere^{3,10}. Replacing potassium with caesium increased transmission 3–100-fold. Facilitation was reduced to 18–61% of its control value. On

reducing the free calcium and increasing the free magnesium concentration, transmission is reduced and facilitation restored, the degree of restoration of facilitation corresponding roughly to the extent to which transmission is restored to the control level.

Caesium and low-calcium-high-magnesium might have unexpected effects on spike duration that would argue against our interpretation of their effects on facilitation. From previous observations of n.t.ps at different frequencies³, we would expect the presynaptic action potential duration to be unchanged at 10 Hz relative to 1 Hz in each of the experimental solutions. Figure 1a–c confirms this prediction. Moreover, we would expect the n.t.p. to be prolonged in the presence of caesium and that this effect would not be reversed by reducing the calcium-to-magnesium concentration ratio. This expectation was also confirmed (Fig. 1d).

To generalise our results, we chose to determine whether similar effects would be obtained using a different pharmacological agent, 4-aminopyridine (4-AP), for blocking potassium channels¹⁷. To obtain effects quantitatively comparable with those of 5.4 mM caesium, we treated neuromuscular junctions with 25 μ M 4-AP (the half-maximal dose of 4-AP is about 50 μ M). We found that, as with caesium, facilitation was markedly reduced and that this effect was largely reversed by reducing the calcium concentration. The results of six experiments are included in Table 1.

In squid axon, both caesium and 4-AP block voltage-dependent potassium channels^{17,18}. In molluscan somata, caesium blocks voltage-dependent channels, whereas 4-AP has little effect on delayed potassium activation^{19,20}. Neither agent seems to block calcium-activated potassium channels. It seems probable that both agents are blocking a voltage-dependent potassium current in crayfish nerve terminals. As these voltage-dependent channels are thought to be the ones subject to progressive inactivation in dorid nudibranchs⁷, blocking them should prevent spike broadening, especially in low calcium, because a persistent calcium current is also required for the expression of progressive spike broadening⁷. The large facilitation we observe in the presence of either 4-AP or caesium and reduced calcium argues against the notion that spike broadening due to potassium inactivation is responsible for synaptic facilitation. Rather, the block of facilitation by caesium and 4-AP in normal calcium seems to be due to a saturation of transmitter release.

There is also some evidence that in snails the calcium-activated potassium channels are subject to inactivation, contributing to spike broadening⁶. Such channels should not be activated in low calcium^{6,21}. The persistence of facilitation in 1 mM calcium in these experiments and even in 10 and 100 μ M calcium (ref. 16 and R.S.Z., unpublished) argue against the notion that spike broadening due to inactivation of calcium-activated potassium channels could underlie facilitation. Thus, it seems that facilitation at crayfish neuromuscular junctions arises in some step of depolarisation-secretion coupling following the generation of the presynaptic electrical signal.

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ROBERT S. ZUCKER
LUIS O. LARA-ESTRELLA

Department of Physiology-Anatomy,
University of California,
Berkeley, California 94720

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Striatal dopamine receptors become supersensitive while rats are given trifluoperazine for six months

OF all the side effects of drugs used to treat psychotic illness such as schizophrenia, chronic tardive dyskinesias are the most disturbing. They appear after months or years of therapy, and persist when the offending drug is withdrawn in about half of the cases. Such tardive dyskinesias resemble in character abnormal movements produced by levodopa in patients with Parkinson's disease. They can be controlled by drugs that antagonise the cerebral actions of dopamine (DA)¹. Thus, they seem to be due to overstimulation of cerebral DA receptors, although they are caused by neuroleptic drugs which, in acute experiments, antagonise cerebral dopamine receptor action^{2–5}. This paradox has been resolved by suggesting that such drugs, by binding to and antagonising cerebral DA receptors, may eventually actually increase receptor sensitivity. Indeed, following treatment for a few weeks with phenothiazines or butyrophenones and withdrawal of the drugs, animals exhibit behavioural and biochemical supersensitivity to DA agonists which persists for some time^{6–13}. However, tardive dyskinesias usually appear while the patient continues to take the offending drug, so we have studied the effect on striatal DA receptor activity of continuous 6-month administration to rats of a potent neuroleptic drug in common clinical use (trifluoperazine). At intervals the animals were tested for a behavioural response to the DA agonist apomorphine, and were killed for biochemical assessment of DA turnover and DA receptor activity in that part of the brain (the corpus striatum) believed to be the site responsible for production of tardive dyskinesias. We report here that the initial behavioural and biochemical evidence for striatal DA receptor blockade by trifluoperazine disappears within a few weeks of starting therapy, to be replaced by supersensitivity after 6 months of drug administration, despite continued drug intake.

Trifluoperazine hydrochloride (Smith, Kline and French, 2.5–3.5 mg per kg per day) was administered in the drinking water for 6 months to male Wistar rats (Olac International; weight 200 g at the start of the experiment). The drug was made up freshly in distilled water (with ascorbic acid added) each day in darkened bottles. Control animals were given distilled water alone. All animals were kept in the same environment at 21 ± 3 °C on a regular 12-h night/day cycle. At intervals (usually after 1 and 2 weeks, and 1, 3 and 6 months) trifluoperazine-treated and control rats were assessed as follows: spontaneous locomotor activity, presence of catalepsy, and incidence and severity of stereotypy induced by apomorphine hydrochloride (Evans Medical; 0.5 mg per kg, subcutaneously (s.c.), given 15 min previously). At the same intervals, groups of trifluoperazine-treated and control rats were killed by decapitation and the paired corpus striatum was rapidly dissected out for: (1) fluorimetric assay of DA and its major metabolites homovanillic acid

(HVA) and 3,4-dihydroxyphenylacetic acid (DOPAC) according to the techniques of Westerink and Korf¹⁴; (2) assay of basal and dopamine (1–150 µM) stimulated adenylate cyclase according to the technique of Miller, Horn and Iverson¹⁵ (ED₅₀ values for dopamine stimulation varied between runs due to inherent changes in assay procedure. This problem was overcome by running control samples and samples from drug-treated animals in strict parallel throughout the entire procedure. Attention should be paid to changes in relative ED₅₀ values); (3) specific ³H-spiperone (20 Ci mmol⁻¹; Radiochemical Centre) binding using a range of 0.125–4.0 nM radioactive ligand and DA (10⁻⁴ M) to define specific binding as modified from the technique of Creese, Burt and Snyder¹⁶ (³H-spiperone binding in striatum is to DA receptors¹⁷). The results are summarised in Tables 1 and 2.

After an initial period of weight loss in the first week the animals tolerated trifluoperazine well, gaining weight at the same rate as control animals, and regularly ingesting between 2.5 and 3.5 mg per kg per d. For the first week spontaneous locomotion was reduced in trifluoperazine-treated animals which were moderately cataleptic. Thereafter, these rats regained normal activity and catalepsy disappeared so that they were indistinguishable in these respects from control animals by 3 and 6 months.

Administration of apomorphine (0.5 mg per kg, s.c.) to control animals at each testing period resulted in the production of mild stereotypy consisting of discontinuous locomotion and sniffing. Apomorphine-induced stereotypy was antagonised by trifluoperazine in the first month of treatment, had returned to control levels by 3 months, and was enhanced after 6 months, the animals exhibiting pronounced licking, gnawing or biting (Table 1). From consideration of the proportion of animals continuously exhibiting this behaviour to a range of doses of apomorphine (0.125–2.0 mg per kg, s.c.), the dose-response curve for maximum stereotypy was found to have shifted to the left in the animals treated with trifluoperazine for 6 months, compared with control rats.

Striatal DA concentrations were unaltered, whereas HVA and DOPAC levels were increased in the first weeks of drug administration, but thereafter the increase in DA metabolites in the striatum disappeared rapidly, as has been observed by others^{18,19} (Table 1). Striatal DA, HVA and DOPAC levels after 6 months of trifluoperazine treatment were not consistently different from those in age-matched control rats.

The increase in striatal adenylate cyclase activity from basal levels was linearly related to added DA concentration between 1 and 150 µM in control animals. In the first month of trifluoperazine intake, more DA was required to produce an equivalent stimulation of striatal adenylate cyclase above basal

Table 1 Effect of 6 months of oral administration of trifluoperazine (2.5–3.5 mg per kg per d) on apomorphine-induced stereotypy and striatal DA, HVA and DOPAC levels compared with controls

Time	Apomorphine-induced stereotypy score		% Of control		
	Control group	Drug-treated group	DA	HVA	DOPAC
1–2 weeks	2.0 ± 0.0	0.0 ± 0.0†	98 ± 7	234 ± 31†	238 ± 34†
1 month	2.0 ± 0.0	0.3 ± 0.2†	—	122 ± 15	107 ± 9
3 months	2.3 ± 0.2	1.6 ± 0.2	130 ± 9*	124 ± 5	88 ± 5
6 months	2.1 ± 0.1	3.1 ± 0.1*	89 ± 10	118 ± 7	110 ± 10

Apomorphine-induced (0.5 mg per kg, s.c.) stereotypy was assessed according to the scale: 0 = normal; 1 = continuous locomotor activity, discontinuous sniffing; 2 = discontinuous locomotor activity, continuous sniffing; 3 = discontinuous biting, licking and gnawing; 4 = continuous biting, licking or gnawing. The results are expressed as the mean ± 1 s.e.m. for 6 animals in each group. Striatal DA, HVA and DOPAC levels were measured by the technique of Westerink and Korf¹⁴, in 6 control and 6 drug-treated animals at the time intervals shown. Values for control animals for DA were 11.1 ± 0.9 µg per g, for HVA 493 ± 57 ng per g and for DOPAC 850 ± 63 ng per g (± 1 s.e.m.) and showed no change with age. All data obtained in drug-treated animals were compared with those obtained in age-matched untreated animals kept in otherwise identical conditions and studied at the same time.

* $P < 0.05$; † $P < 0.005$, compared to control animals; Student's *t* test for parametric data; Mann-Whitney *U* test for non-parametric results.

Table 2 Effect of 6 months of oral administration of trifluoperazine (2.5–3.5 mg per kg per d) on dopamine stimulation of striatal adenylyl cyclase and specific striatal ^3H -spiperone binding compared with controls

Time	Stimulation of adenylyl cyclase caused by 50 μM MDA (pmol cAMP per 2 mg tissue per 2.5 min)		Total specific ^3H -spiperone binding (pmol per mg)	
	Control group	Drug-treated group	Control group	Drug-treated group
1–2 weeks	107 $\pm$ 6	41 $\pm$ 19*	11.7 $\pm$ 0.4	11.0 $\pm$ 0.5
1 month	106 $\pm$ 18	51 $\pm$ 27*	11.3 $\pm$ 0.8	4.7 $\pm$ 0.2*
3 months	48 $\pm$ 11	61 $\pm$ 19	—	—
6 months	55 $\pm$ 15	90 $\pm$ 24*	8.1 $\pm$ 0.2	9.8 $\pm$ 0.4*

The DA stimulation of striatal adenylyl cyclase caused by 50 μM MDA was calculated from the linear portion of the concentration (1–150 μM MDA)–response curve, obtained by regression analysis, as determined by incubating striatal tissue from 3 control or drug-treated rats with the range of concentrations (each determination being carried out in duplicate). Total specific ^3H -spiperone binding (as judged by DA 10^{-4} M displacement) was determined at nonsaturating substrate concentration (0.25 and 0.50 nM). Data were obtained by using pooled tissue from at least 6 control or drug-treated animals with each determination being carried out in triplicate. All data obtained in drug-treated animals were compared with those obtained in age-matched untreated animals kept in otherwise identical circumstances and studied at the same time.

* $P < 0.005$ Student's t test.

levels, and the concentration of DA required to produce a 50% increase in adenylyl cyclase from basal levels (ED_{50}) was increased to 87 μM , compared with 6 μM in age-matched control animals. After 3 months of trifluoperazine treatment the ED_{50} for added DA was barely different from that required in control animals (66 μM and 72 μM , respectively). However, by 6 months, 50 μM produced a greater percentage increase in adenylyl cyclase from basal level compared with control animals and the ED_{50} was less in drug-treated than control animals (4 μM and 32 μM , respectively) (Table 2).

Total specific binding of ^3H -spiperone to striatal membranes was less in trifluoperazine-treated animals compared with controls at nonsaturating ligand concentrations after 1 month of drug intake (Table 2). Scatchard analysis showed this to be due to a reduced receptor affinity in animals treated for 1 month relative to control animals (K_D 1.04 $\pm$ 0.13 nM compared with 0.32 $\pm$ 0.05 nM for controls; $P < 0.005$) whereas total number of binding sites was unchanged (15.1 $\pm$ 1.7 pmol per g compared with 19.3 $\pm$ 2.6 pmol per g for controls). However, by 6 months total specific ^3H -spiperone at ligand concentrations below saturation was greater in trifluoperazine-treated than in age-matched control animals (Table 2). Scatchard analysis revealed a tendency for increased receptor affinity (K_D 0.50 $\pm$ 0.06 nM compared with 0.67 $\pm$ 0.06 nM for control animals; $P < 0.08$) with no obvious change in number of binding sites (23.4 $\pm$ 2.5 pmol per g, and 20.7 $\pm$ 1.7 pmol per g for control animals).

The effect of chronic trifluoperazine administration in increasing receptor affinity was seen more clearly by comparing the dissociation constant after 1 month of drug treatment with that after 6 months (K_D 1.04 $\pm$ 0.13 nM and 0.50 $\pm$ 0.06 at 1 and 6 months, respectively; $P < 0.01$). Thus, the dissociation constant fell between 1 and 6 months of trifluoperazine therapy, indicating an increased receptor affinity. By contrast, in control animals the reverse occurred, namely an increased dissociation constant (K_D 0.32 $\pm$ 0.05 nM and 0.67 $\pm$ 0.06 nM at 1 and 6 months, respectively; $P < 0.007$), indicating a decrease in receptor affinity with normal ageing. Furthermore, comparison of the number of binding sites at 1 and 6 months shows a small increase in trifluoperazine-treated animals (15.1 $\pm$ 1.7 pmol per g to 23.4 $\pm$ 2.5 pmol per g; $P < 0.02$), but no change in control animals (19.3 $\pm$ 2.6 pmol per g, 20.7 $\pm$ 1.7 pmol per g; $P < 0.05$).

Our results indicate a fairly consistent general conclusion, supported by a variety of different behavioural and biochemical

observations. Trifluoperazine administration in the first days or weeks caused antagonism of DA-mediated behaviour, a compensatory increase in striatal DA turnover, decreased capacity for DA stimulation of striatal adenylyl cyclase, and decreased striatal ^3H -spiperone binding. All these effects may be attributed to the acute or subacute actions of these drugs in brain tissue. Even the *in vitro* adenylyl cyclase and ^3H -spiperone binding changes probably represent the effects of residual drug in the tissues studied.

Over the next 6 months, trifluoperazine intake remained constant and we assume that brain levels of the drug did not change in a manner that might explain our results. A gradual accumulation of the drug might occur, but not a progressive decrease in brain trifluoperazine concentration. In any event, the changes we observed over the months of chronic trifluoperazine intake were not a gradual decline of the acute effects of the drug, but a reversal of these actions.

After 1 to 3 months of trifluoperazine intake, spontaneous locomotor activity, stereotypy induced by apomorphine, striatal dopamine turnover, adenylyl cyclase and ^3H -spiperone binding had all returned to normal, as judged by age-matched control rats. After 6 months, drug-treated animals were supersensitive to apomorphine in respect of maximum stereotypy, and showed increased sensitivity for DA stimulation of striatal adenylyl cyclase and increased ^3H -spiperone-defined receptor affinity. Thus, a drug which initially caused behavioural and biochemical antagonism of DA effects in the striatum produced the opposite effects after 6 months' administration, namely enhanced striatal dopamine action.

Does this reversal of trifluoperazine's effect on DA action during chronic administration apply to other neuroleptic drugs? So far, we have obtained similar results for thioridazine (30–40 mg per kg per day) showing a reversal of all these behavioural and biochemical indices of DA action over 6 months' intake.

We consider our results to indicate that as far as striatal DA receptors are concerned, the initial antagonistic effects of neuroleptic drugs are replaced by some evidence of supersensitivity despite continued drug intake. This would explain why drug-induced parkinsonism may disappear and why tardive dyskinesias may appear during chronic neuroleptic therapy. Furthermore, this type of experiment may provide a realistic animal model for testing drugs for their capacity to induce tardive dyskinesias.

Finally, it is reasonable to wonder whether changes similar to those observed in the striatum occur in the mesolimbic and mesocortical DA areas of the brain. If so, and the necessary experiments are under way in our laboratories to study this, the whole concept of antipsychotic drugs acting primarily by antagonising cerebral DA receptors may require revision.

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A. CLOW
P. JENNER
A. THEODOROU
C. D. MARSDEN

University Department of Neurology,
Institute of Psychiatry and King's College
Hospital Medical School,
Denmark Hill, London SE5, UK

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Effects of insulin on insulin-binding components extracted from rat fat cell membranes

We have previously shown the presence of two distinct insulin-binding species (designated species I and II) in a Triton X-100 extract of fat cell membranes¹. They were separable electrophoretically and displayed different insulin-binding characteristics. Scatchard analysis of the insulin-binding data for species I indicated a complex, non-classical association, illustrated by a curvilinear plot. The characteristics of this curve are quite similar to those of the binding curves obtained with unfractionated detergent extract or with intact plasma membranes¹. In contrast to these findings, insulin-binding to species II was characterised by a relatively low affinity and a linear Scatchard plot. Although these findings indicate different insulin-binding and electrophoretic properties for the two species, the possibility remains that they are related structurally. For example, the K_d values for the low-affinity portion of the peak I Scatchard plot and for insulin binding to peak II are similar. Furthermore, following the incubation of isolated peak I with ¹²⁵I-labelled insulin and subsequent electrophoresis, a small amount of radioactivity migrated in the position of peak II (ref. 1). We report here that interconversion occurs between the two binding species and that the conversion is mediated by insulin.

Soluble insulin-binding material was prepared by treating a purified plasma membrane fraction from rat adipocytes with 1% (v/v) Triton X-100 as described by Cuatrecasas². Particulate material was removed by centrifugation of the detergent suspension at 150,000g for 90 min at 4 °C. ¹²⁵I-labelled insulin (100-200 µCi per µg) was prepared by the chloramine-T

method as previously described¹. Specific binding of ¹²⁵I-labelled insulin by the soluble material was measured by the polyethylene glycol precipitation method², modified slightly to increase the sensitivity of the assay¹.

The soluble insulin-binding material was subjected to electrophoresis on 5% polyacrylamide gels (12.5 cm in length) using the system of Davis³ in conditions we have previously reported¹. Following the electrophoresis, the gel was sliced and the insulin-binding activity eluted from the various gel segments as previously described¹. The binding activity was assayed in the eluates by the polyethylene glycol method² and then concentrated by ultrafiltration¹. Results were obtained using the concentrated preparations of the isolated insulin-binding species.

In the absence of insulin at 4 °C the two insulin-binding species migrate as single peaks of activity with no spontaneous formation of any significant amounts of the other binding species (Fig. 1). Thus, in these conditions both binding species are relatively stable and migrate during electrophoresis in their customary positions.

Figure 2 shows the effect of insulin. Addition of the hormone (50 ng ml⁻¹) to isolated species I (Fig. 2a) causes the appearance of a significant amount of insulin-binding material in the region occupied by species II. There is a small amount of insulin-binding material located beyond species II but this small insulin-binding peak is inconsistently found and its significance is not clear. In contrast to the findings with isolated species I, addition of insulin to isolated species II (Fig. 2b) fails to cause any formation of species I.

Figure 3 shows the sensitivity of the insulin-mediated conversion of species I to species II. The loss of insulin-binding activity in the position of species I is paralleled by the appearance of insulin-binding activity in species II. A significant amount of conversion is elicited by an insulin concentration as low as 0.5 ng ml⁻¹, which is well within physiological insulin levels. These findings demonstrate a direct and highly sensitive effect of insulin on conversion of the high-affinity insulin-binding activity to a low-affinity form which has a different electrophoretic mobility from the parent species.

Ginsberg *et al.*⁴, using material solubilised from turkey red cells, showed an insulin-mediated conversion of a large-form insulin-binding peak to a small form. These studies differ from our findings in rat fat cells by the demonstration that the small peak could be converted back to the large peak by insulin. Our failure to find a conversion of species II to species I either in the presence or absence of insulin may be related to the conditions used. All procedures were carried out at 4 °C, a temperature at

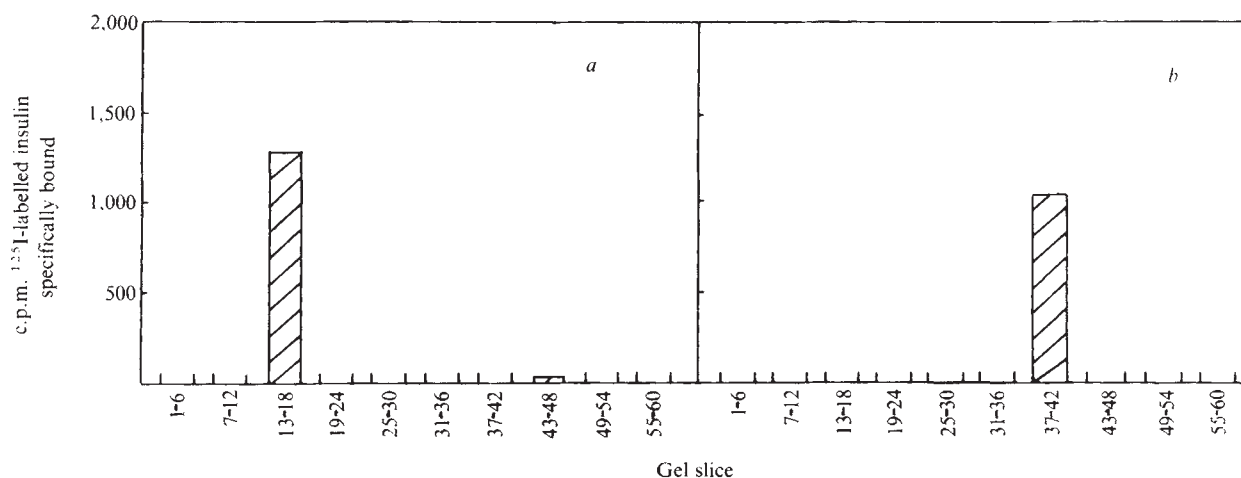


Fig. 1 Electrophoresis of the isolated insulin-binding species. Species I (a) and species II (b) were isolated from the Triton X-100 extract of fat cell plasma membranes and concentrated as previously described¹. Polyacrylamide-gel electrophoresis of the separate species was carried out for 3 h following a 16 h incubation period at 4 °C (ref. 1). The gels were then sliced and the gel sections eluted¹. The amount of insulin-binding activity was determined in the eluates by the polyethylene glycol method⁴.

which changes in the size of the turkey insulin receptor are slow⁴. However, the insulin-binding species isolated from rat fat cells may be quite different from those isolated from turkey red cells. For example, we isolate two insulin-binding species from the adipocyte plasma membrane, whereas only one species is initially isolated from the avian cell. In the red cell system, insulin exposure is needed before the second species is detected. The fat cell system also seems to be more sensitive to the effects of insulin. A hormone concentration of 0.5 ng ml^{-1} causes significant binding-species conversion ($\sim 30\%$) in our system, whereas a concentration of 25 ng ml^{-1} is required in the red cell extract to effect an 18% conversion to the small form. Moreover, $\sim 70\%$ of the total binding by adipocyte species I could be converted to species II, compared with a 30% conversion for the avian cell preparation.

Maturo and Hollenberg⁵ have shown the presence of two distinct insulin-binding activities in a detergent extract of rat liver plasma membrane. The large species isolated by Sepharose-6B chromatography had complicated insulin-binding properties, whereas the smaller form demonstrated a simple insulin-binding isotherm. The insulin-binding activity isolated by insulin-agarose affinity chromatography corresponded to the small form, and the large form was regenerated by the addition of a glycoprotein fraction which by itself failed to bind insulin specifically. The two forms of insulin-binding activity may correspond to species I and II which we have also demonstrated in rat liver membranes⁶. Maturo and Hollenberg⁵ and Jacobs *et al.*⁷ have found that the insulin-binding activity isolated by insulin-agarose affinity chromatography of liver extracts has hormone-binding characteristics which are similar to the binding characteristics of species II (ref. 1). This observation supports our present finding that the interaction of species I (high affinity, complex insulin-binding kinetics) with insulin (and presumably with insulin-agarose) causes a conversion to species II (low

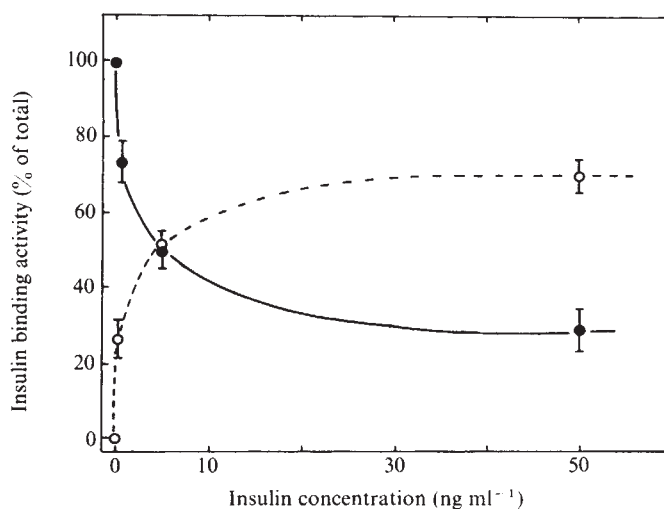


Fig. 3 Effect of insulin on the conversion of species I to species II. Species I was incubated with the indicated concentrations of insulin for 16 h at 4°C . The amount of species II formed and the disappearance of species I were determined as indicated in Fig. 1. Each point on the graph represents the mean $\pm$ s.e. of three separate experiments. The total amount of insulin-binding eluted from the gel was taken as 100%. ●, Peak I; ○, peak II.

affinity, simple insulin-binding kinetics). Thus, it may be impossible to purify the high-affinity insulin-binding component by the use of affinity columns that contain insulin.

The findings reported here emphasise the complexities which surround the interaction of insulin with the insulin receptor. The ability of insulin to induce the formation of a low-affinity component from a high-affinity binding species may help to explain the mechanisms underlying certain phenomena found in studies of insulin-binding to target cells and subcellular particles (for example, 'negative cooperativity', curvilinear Scatchard plots). In addition, the loss of insulin binding associated with high plasma insulin levels in insulin-resistant states of man and animals⁸⁻¹³ may in part be related to the insulin-mediated conversion of species I to species II. Finally, as the conversion of the high-affinity species to the low-affinity form is highly sensitive to insulin, this molecular event may represent an early step in insulin action which follows binding of the hormone to the receptor.

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MICHAEL N. KRUPP
JAMES N. LIVINGSTON

*Endocrine-Metabolism Unit,
Department of Medicine and of Biochemistry,
University of Rochester School of Medicine,
601 Elmwood Avenue, Rochester, New York 14642*

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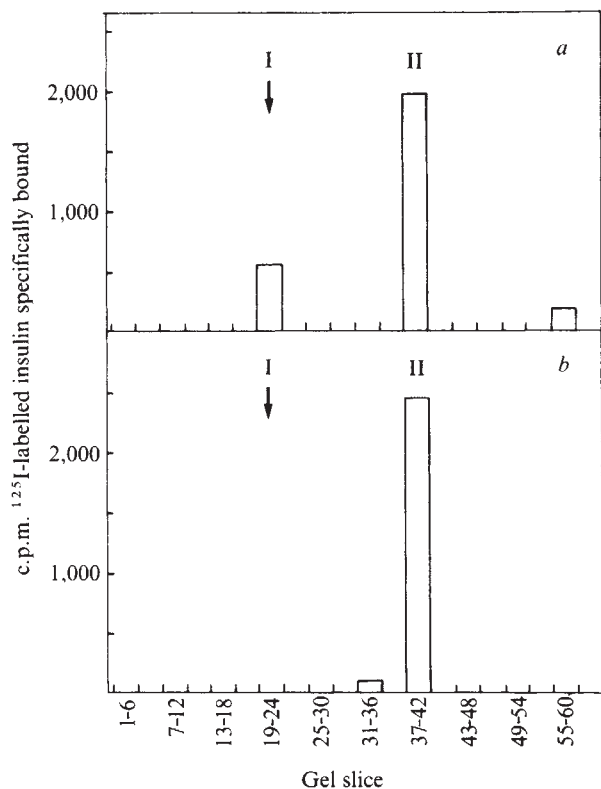


Fig. 2 Electrophoresis of the isolated insulin-binding species in the presence of insulin. Species I (a) and species II (b) were incubated with insulin (50 ng ml^{-1}) for 16 h at 4°C . The incubation mixtures were then subjected to gel electrophoresis for 3 h. The gels were cut, eluted and insulin-binding activity determined as described in Fig. 1.

Effect of tetrodotoxin on adrenaline secretion in the perfused rat adrenal medulla

EXTRACELLULAR Ca ions are required for the secretion of many hormones¹. In the adrenal medulla catecholamine secretion evoked by acetylcholine (ACh) is completely blocked by omitting Ca from the medium^{2,3}. The mechanism for Ca entry has been studied in a pheochromocytoma cell line (PC12) which originated from a rat adrenal medullary tumour^{4,5}. Two alternative ways of Ca entry were considered, voltage-dependent Ca channels and ACh channels. By analysing dopamine secretion⁴ and radioactive Ca influx⁵ in various ionic environments it was concluded that more than 80% of the Ca ions enter through the voltage-dependent Ca channels during stimulation by cholinergic agonists. Action potentials mediated by Na^{6,7} and Ca⁷ have been demonstrated in primary cultures of rat adrenal chromaffin cells. Furthermore, extracellular recording techniques can detect spontaneous action potentials in these cells. The frequency of these action potentials is modified in a dose-dependent manner by ACh (3×10^{-7} M to 10^{-4} M)⁷, and the same range of ACh concentrations stimulates adrenaline secretion in the perfused rat adrenal medulla^{3,8}. Thus modulation of action potential frequency by ACh may be a mechanism for controlling the amount of Ca entry. The potential change during a Na action potential may activate voltage-dependent Ca channels, thereby increasing Ca influx. Also, spontaneous action potentials are blocked by 6 μ M tetrodotoxin (TTX)⁷, a specific inhibitor of Na channels⁹. Therefore, if the Na action potential is involved in secretion, TTX should reduce Ca entry which in turn should decrease catecholamine secretion. We tested this hypothesis in the perfused rat adrenal medulla and report here that TTX decreases a portion of the adrenaline secretion evoked by either high KCl or ACh. Thus, Na action potentials, by facilitating the activation of Ca channels, may be involved in the regulation of adrenaline secretion from adrenal chromaffin cells.

The technique for perfusion of the isolated rat adrenal medulla has been described by Ishikawa *et al.*³. Male rats (Sprague-Dawley, Simonsen) weighing 180–300 g were used. After ether anaesthesia an adrenolumbar vein entering the left adrenal gland was cannulated and arteries and other veins connected to the gland were ligated. The adrenal gland was then removed from the rat and maintained at room temperature

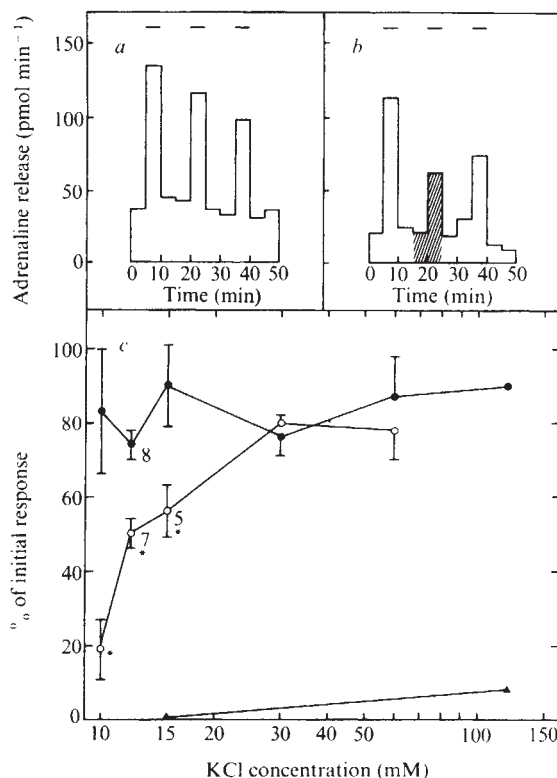


Fig. 1 *a*, Adrenaline release during three successive challenges with 12 mM KCl (indicated by the horizontal bars at the top of the graph). Each KCl challenge and collection period lasted 5 min. Between KCl challenges the adrenal was perfused for 10 min with normal saline. At the end of the 50 min the adrenal gland was discarded. *b*, Effect of TTX (6 μ M) on adrenaline release stimulated by 12 mM KCl. Conditions were identical to those of *a* except that TTX was present during the period indicated by cross hatching. *c*, Effects of TTX and CoCl_2 on adrenaline release stimulated by different concentrations of KCl. Experiments were carried out as for *a* and *b*. After subtraction of basal secretion from the stimulated secretion, the net response to the second KCl challenge is expressed as per cent of the net response to the initial KCl challenge. The second challenge contained KCl alone ($\bullet$), 6 μ M TTX ($\circ$) or 5 mM CoCl_2 ($\blacktriangle$). The KCl concentration is plotted on a log scale. Each point with standard error bars represents the mean result from four different adrenals, except where *n* is specified by the number adjacent to the point. Points without standard error bars represent single samples. Statistically significant (Student's *t*-test two-tailed, $P < 0.05$) depression of the KCl response in the presence of TTX is indicated by an asterisk. This effect of TTX was completely reversible, as there were no statistically significant differences ($P > 0.07$) in response to the third KCl challenge between controls and adrenals which received TTX during the second challenge. The effect of 6 mM CoCl_2 was also reversible. CoCl_2 (5 mM) interfered with neither the extraction nor fluorescence of adrenaline standards or samples.

Table 1 Adrenaline release stimulated by acetylcholine (2 μ M)

Conditions during second response	Second response as a % of initial response	Third response as a % of initial response
ACh control	71 $\pm$ 3 (6)	47 $\pm$ 11 (6)
ACh + TTX (6 μ M)	41 $\pm$ 4 (6)	37 $\pm$ 6 (6)
	$P < 0.0001^*$	NS at $P = 0.2^*$
ACh + CoCl_2 (5 mM)	0 (2)	216 (2)

Experiments were carried out as described in the legend to Fig. 1*a, b* except that release was evoked by three successive challenges to 2 μ M ACh. The response to the second ACh challenge is expressed as per cent of the response to the initial ACh challenge after subtraction of basal secretion. As in Fig. 1, the first and third responses were evoked in ACh alone. The total initial response to ACh was 120 ± 13 (14) pmol min⁻¹. The basal secretion was 30 ± 7 (14) pmol min⁻¹. Data are expressed as the mean $\pm$ s.e.m. with the number of adrenals indicated in parentheses. The inhibition of the response to ACh by TTX (second response) was reversible (third response). The response to ACh after removal of CoCl_2 was not only reversed but inexplicably enhanced. To prevent ACh hydrolysis eserine sulphate (1.5 μ M) was added to the saline of all experiments involving ACh.

*Student's *t*-test for comparison with the ACh control, two-tailed.

(21–23°C). Saline was perfused retrogradely through the vein and collected from a small incision on the adrenal cortex. The speed of perfusion was controlled by a roller pump at 0.15 ml min⁻¹.

Adrenaline was assayed by using a trihydroxyindole fluorometric assay¹⁰ after previous butanol extraction¹¹ of the samples. The assay is specific for adrenaline. All values are based on adrenaline standards which were carried through the entire extraction procedure for each experiment.

The normal solution used for perfusion had the following composition: NaCl 150 mM, KCl 5.6 mM, CaCl_2 2 mM, MgCl_2 1 mM, NaH_2PO_4 1 mM, HEPES-NaOH 10 mM and glucose 5.0 mM. The saline was bubbled with 100% O₂. The concentration of KCl, CaCl_2 or CoCl_2 was increased by replacing an osmotically equivalent amount of NaCl.

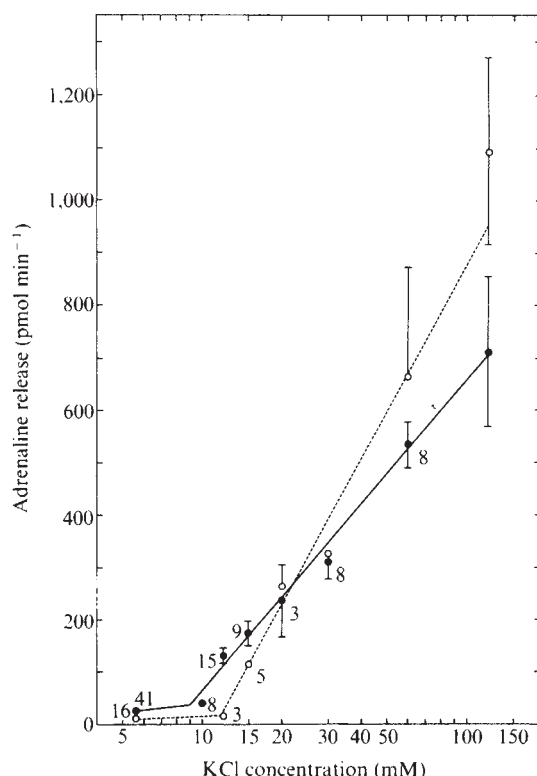


Fig. 2 Adrenaline secretion by adrenals perfused with different concentrations of KCl and the effect of Ca. The duration of the KCl challenge and each collection period was 5 min. Adrenaline secretion at each concentration of KCl (plotted on a log scale) is presented as the amount of secretion per min. The concentration of CaCl_2 was 2 (●) or 10 mM (○). Each point represents the mean result from four different adrenals unless a different n is indicated by the number adjacent to the point. Standard error bars are also given for each point except when the standard error is less than 10 pmol min^{-1} . Basal secretion is represented by the amount of release at 5.6 mM KCl. The lines were drawn by eye.

When the adrenal gland was perfused with normal saline the basal secretion varied between preparations from <15 to $68.8 \text{ pmol min}^{-1}$ ($24 \pm 2 \text{ pmol min}^{-1}$, mean $\pm$ s.e.m., $n = 41$). As the saline was switched to a high KCl solution the rate of adrenaline secretion increased, then returned to the original level when the perfusion solution was returned to normal saline. However, the rate of adrenaline output tended to decrease as the KCl challenges were repeated (Fig. 1a). The rate of adrenaline secretion was greater with higher concentrations of KCl (Fig. 2). The lowest concentration to evoke adrenaline secretion above the basal level was $\sim 10 \text{ mM KCl}$ ($39 \pm 7 \text{ pmol min}^{-1}$, $n = 8$). The adrenaline release increased exponentially to 120 mM KCl ($610 \text{ pmol min}^{-1}$ for a 10-fold KCl concentration change).

CoCl_2 (5 mM), a blocker of voltage-dependent Ca channels¹², inhibited KCl-evoked release. The inhibiting effect of CoCl_2 on KCl-stimulated catecholamine secretion has been previously reported^{4,13,14} and suggests that voltage-dependent Ca channels mediate Ca entry.

An increase in the external Ca concentration raises the activation threshold for voltage-dependent Na and Ca channels¹⁵⁻¹⁸. If voltage-dependent channels are involved in KCl-induced catecholamine secretion, raising the external Ca should shift the lowest concentration of KCl required to evoke secretion. When CaCl_2 in the saline was increased from 2.0 mM to 10 mM, the lowest KCl concentration necessary to evoke adrenaline secretion shifted from $\sim 10 \text{ mM}$ to between 12 and 15 mM and the slope increased to $940 \text{ pmol min}^{-1}$ for a 10-fold KCl change (Fig. 2, dotted line). The greater slope in 10 mM Ca saline can be explained by an increased Ca influx through individual Ca channels. The shift in the lowest KCl concen-

tration was significant, as the rate of secretion was lower at 12 and 15 mM KCl in the presence of high Ca.

The participation of Na action potentials in KCl-evoked secretion is demonstrated by the effect of TTX on secretion. When $6 \mu\text{M TTX}$ was added to saline, KCl-evoked release was depressed to various degrees depending on the concentration of KCl (Fig. 1b, c). The effect of TTX is present only at lower KCl concentrations.

High KCl concentrations may stimulate the cholinergic nerve terminals which innervate the chromaffin cells and thus cause subsequent release of ACh. To rule out such an indirect effect of KCl, atropine sulphate ($10 \mu\text{M}$), an inhibitor of the muscarinic ACh receptor, was added to the saline of all experiments involving stimulation by KCl. This concentration of atropine completely blocked stimulation of adrenaline release by $2 \mu\text{M ACh}$ (see below) and ACh stimulation of action potential frequency⁷. Although the ACh receptors on the rat adrenal chromaffin cell are predominantly of the muscarinic type⁷, we have additionally confirmed that the above inhibitory effects of TTX on 15 mM KCl stimulation of adrenaline secretion still occur in the presence of high concentrations of both nicotinic and muscarinic antagonists ($500 \mu\text{M}$ hexamethonium bromide + $50 \mu\text{M}$ atropine sulphate). Thus, the effect of TTX on KCl-evoked adrenaline release is due to a direct effect on the chromaffin cell.

The natural stimulus for adrenaline secretion is ACh, which is released from the splanchnic nerve terminals. We therefore tested the effects of TTX on ACh-stimulated secretion. The addition of ACh ($2 \mu\text{M}$) to saline increased adrenaline secretion ($120 \pm 13 \text{ pmol min}^{-1}$, $n = 14$). This ACh-stimulated secretion was blocked by $10 \mu\text{M}$ atropine sulphate. When TTX ($6 \mu\text{M}$) was added with ACh ($2 \mu\text{M}$), the rate of adrenaline secretion decreased to 58% of the control (Table 1).

A TTX-resistant, Co-sensitive component of adrenaline secretion is present during stimulation by $2 \mu\text{M ACh}$ and at all levels of KCl stimulation. This stimulation is probably due to the direct activation of voltage-dependent Ca channels. The effect of TTX on KCl-evoked and ACh-stimulated adrenaline secretion is likely to be due to the specific effect of TTX on Na channels in the chromaffin cells. One possible explanation for the involvement of Na channels in secretion is that Ca ions may enter the cell through the Na channel¹⁹. As CoCl_2 (5 mM), an inhibitor of the Ca^{12} but not the Na^{20} channel, completely blocked KCl (Fig. 1c) and ACh (Table 1) stimulated release, this seems unlikely. Alternatively, a relatively small depolarisation by KCl or ACh probably stimulates Na action potentials which serve as a trigger for further activation of voltage-dependent Ca channels.

The effect of TTX on secretion is larger at low KCl concentrations and is eventually lost as the KCl concentration is increased. One explanation is provided by the different inactivation characteristics of the Na and Ca channels. Voltage-dependent inactivation of Na channels generally occurs rapidly²¹ whereas Ca channels allow Ca ions to enter the cell for prolonged periods even at large levels of sustained depolarisation^{22,23}. Second, analysis of cultured adrenal chromaffin cells indicates that a low concentration of KCl (11.6 mM) produces an increase in action potential frequency which is sustained for the duration of perfusion with KCl (5 min). With a higher concentration of KCl (15.6 mM) the frequency of action potentials increases initially then gradually decreases to basal level⁷. Thus, the total number of action potentials during a 5-min application of KCl may be larger in 11.6 mM than in 15.6 mM KCl. Such an effect would diminish the relative contribution of the Na action potential as the KCl concentration is increased.

These results conflict with previous studies on the PC12 clonal cell line. PC12 cells have Ca^{24} and Na^{24} (TTX-sensitive) action potentials. Although direct activation of voltage-dependent Ca channels by KCl or cholinergic agonists contribute to Ca influx⁵ and catecholamine secretion^{4,25}, TTX has no effect on either Ca influx or secretion. As cells *in situ* may be under the influence of

circulating hormones and other factors²⁶, their properties could be different from those of cell lines.

In view of the partial inhibition by TTX of adrenaline release from the adrenal medulla we propose that the Na action potential, by facilitating the activation of a voltage-dependent Ca channel, may have a regulatory role in catecholamine release. The facilitatory effect of the Na action potentials on secretion occurs at the level of the chromaffin cell membrane and is only detectable at low levels of depolarisation. Na action potentials seem to have a similar function in pancreatic β cells, where 37% of the first phase of insulin secretion evoked by glucose is blocked by TTX²⁷.

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Y. KIDOKORO
A. K. RITCHIE

The Salk Institute,
PO Box 1809,
San Diego, California, 92112

S. HAGIWARA

Department of Physiology,
University of California at Los Angeles,
Los Angeles, California, 90024

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Thyroid hormone induces competence for oestrogen-dependent vitellogenin synthesis in developing *Xenopus laevis* liver

OESTROGEN-DEPENDENT synthesis of vitellogenin, the precursor of the major yolk proteins, in the liver of egg-laying vertebrates is an attractive model for investigating regulatory mechanisms of gene expression. Induction of vitellogenin synthesis by oestrogen occurs not only in the liver of females but also in males which normally do not produce this protein, thus showing that responsiveness to oestrogen is not linked to sex (for review see ref. 1). However, Follet and Redshaw² reported that

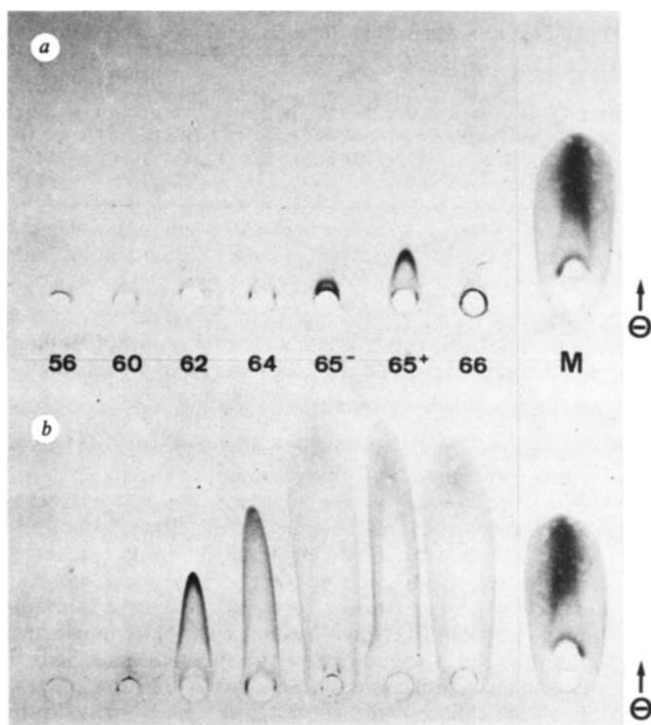


Fig. 1 Detection of vitellogenin in the blood plasma of normal *Xenopus* larvae by rocket immunoelectrophoresis. Blood samples were collected from normal (a) and oestrogen-treated larvae (b) after anaesthesia with 1:1,000 MS 222 solution (Tricaine methanesulphonate, Sandoz) by heart puncture. For each stage, designated according to the scale of Nieuwkoop and Faber⁶, 3-5 μ l samples from four or five animals were pooled, centrifuged and the plasma used for immunological analysis. Rocket immunoelectrophoresis⁵ was performed in 55 $\times$ 205 $\times$ 1 mm 1% agarose gel (Sigma) in 0.02 M barbital buffer (pH 8.6) containing monospecific antivitellogenin antibody diluted 1:15. Vitellogenin antibody was raised in a rabbit by subcutaneous injection of 1 mg purified vitellogenin and Freund's complete adjuvant, followed by two boosters of 0.5 mg vitellogenin each 10 and 75 days respectively after the first injection. By adsorbing the rabbit antiserum with blood plasma from unstimulated male *Xenopus* it was possible to eliminate nonspecific components and to obtain a monospecific antivitellogenin antibody, as revealed by the Ouchterlony test (data not shown). To each well 10 μ l of blood plasma, diluted 1:1 with barbital buffer, were applied and electrophoresis performed at 4°C, beginning at 10 V for 30 min, and thereafter at 30 V for 17.5 h; after the initial 2 h 10 μ l of barbital buffer were added to each well. After electrophoresis the gels were washed successively in 0.9% NaCl and distilled water. After drying they were stained for 10 min in 0.5% Coomassie brilliant blue and destained in the solvent containing ethanol, distilled water and acetic acid. Arabic numerals give stages according to Nieuwkoop and Faber⁶, representing prometamorphic tadpoles (56/57), actual transformation of tadpoles (60-66) and postmetamorphic froglets (>66). M, vitellogenin marker. The broad and diffuse rockets obtained with the marker protein and blood plasma of later developmental stages are caused by excessive amounts of vitellogenin and do not reflect heterogeneity of the proteins.

liver of *Xenopus laevis* larvae is refractory to oestrogen, becoming responsive shortly after completion of metamorphosis. As amphibian metamorphosis is known to be obligatorily dependent on thyroid hormones (for review see ref. 3), the question arises as to whether acquisition of competence to respond to oestrogen by the *Xenopus* liver cells is part of the hormonally regulated metamorphic changes or merely reflects age-dependent maturation. Here we present evidence that during normal development of *Xenopus laevis* the liver cells become responsive to oestrogen during metamorphosis and before vitellogenin synthesis has been initiated by endogenous oestrogens. Moreover, we demonstrate that responsiveness to oestrogen fails to appear when metamorphosis is suppressed, but readily appears after induction of metamorphosis by thyroxine, suggesting that acquisition of competence to respond to oestrogen by the *Xenopus* liver cells is controlled by thyroid hormones.

Responsiveness to oestrogen was tested by the appearance of vitellogenin in the blood of tadpoles stimulated by oestrogens either continuously or for periods of 7 days, as described in

Table 1 First appearance of vitellogenin in the blood and of the response to estrogen by liver cells during normal development of *Xenopus laevis*

	Stage; age	Vitellogenin-positive samples as % of (total)					
		56/57; 39	60; 46	62; 49	64; 53	65 ⁻ ; 54	65 ⁺ ; 56
Control		0% (6)	0% (6)	0% (10)	0% (8)	50% (8)	80% (10)
Oestradiol-3, 17 β		0% (3)	0% (3)	15% (6)	100% (5)	100% (3)	100% (8)
Ovocyclin		0% (3)	0% (3)	15% (6)	100% (4)	100% (4)	100% (8)
							≥ 66 ; 58-92
							25% (20)
							100% (15)
							100% (15)

Tadpoles of *Xenopus laevis* were obtained by induced breeding and reared in standard conditions⁷. To test for responsiveness to oestrogen they were exposed either continuously from stage 49 onwards or for 7 days to oestradiol-3,17 β (Merck) or Ovocyclin (1 part oestradiol-3-benzoate + 3 parts oestradiol-3-benzoate-17-acetate) (Ciba-Geigy) each at a concentration of 5 μ g per 100 ml water. Age refers to days after hatching.

Table 1 legend. In agreement with an earlier report of Gallien⁴ continuous treatment resulted in sex reversal in genetic males, yielding 100% females as judged from a random sample of 22 autopsies. Without oestrogen a normal sex ratio of tadpoles was found as shown by the proportion of 32 females:31 males determined in a randomly selected sample. Vitellogenin was detected in the plasma of pooled blood samples by rocket immunoelectrophoresis⁵, as described in Fig. 1 legend.

In a first experiment, involving three different batches of tadpoles, we investigated the first appearance of vitellogenin and the earliest response to oestrogen by the liver in normally developing tadpoles. A typical result of the immunoelectrophoretic analysis is given in Fig. 1, which shows the specificity of the immunological reaction. The height of the precipitation peaks is an indication of the vitellogenin concentration in the blood plasma. The results of these analyses are summarised in Table 1. During normal development and in the absence of extraneous oestrogen vitellogenin is already detectable during

late metamorphosis, stage 65/66 (ref. 6). As there is a 50%:50% distribution of males and females in this group, pooling of blood samples results in a dilution of vitellogenin. Therefore the possibility that synthesis of this protein might be initiated at an even earlier stage is not excluded. In contrast to Follet and Redshaw² who considered the appearance of vitellogenin synthesis to be a post-metamorphic event, our results provide good evidence that this change in biosynthetic activity of the larval liver cells is part of the metamorphic transformation. We assume that this discrepancy might be caused by differences in sensitivity of the methods used for detection of vitellogenin. Figure 1a and Table 1 show reduced levels of vitellogenin and a decrease in the relative number of vitellogenin-positive blood samples from 80% to 25% in newly metamorphosed froglets, suggesting vitellogenin utilisation by the first generation of growing oocytes.

To determine the stage of onset of competence to respond to oestrogen in the larval liver, blood samples of oestrogen-stimulated tadpoles were analysed. Except for sex reversal in genetic males, oestrogen treatment had no visible morphogenetic effect nor did it alter the rate of development. A representative experiment is shown in Fig. 1b, demonstrating precocious appearance and higher levels of vitellogenin in the blood of oestrogen-treated tadpoles. The corresponding results, summarised in Table 1, clearly reveal that oestradiol-3,17 β and Ovocyclin applied in water were equally efficient in inducing vitellogenin synthesis as early as stage 62. No differences in timing and extent of response were noted between continuous and short-term treatment by oestrogen. Thus during normal development of *Xenopus laevis* the competence of larval liver cells to respond to oestrogen is apparently established during metamorphosis, before vitellogenin synthesis has been induced by endogenous oestrogen.

To establish whether the onset of responsiveness of the larval liver to oestrogen is dependent on thyroid hormone, the response to oestrogen was followed in thyrostatic tadpoles, developmentally arrested at stage 56, between 65 and 85 days after hatching and in a group of thyrostatic tadpoles, induced to metamorphosis by L-thyroxine at 78 days after hatching. Typical immunological tests are shown in Fig. 2 and a summary of the analyses is presented in Table 2.

Thyrostatic tadpoles in which metamorphosis had been suppressed failed to respond to effective levels of oestrogen, even at an age when normally developing animals had become fully responsive to this hormone. However, treatment of thyrostatic tadpoles with 10^{-8} M L-thyroxine at an age at which normally-developing sibling larvae (Table 1) had already completed metamorphosis not only induced morphological transformation but also elicited responsiveness to oestrogen. As in normal development, vitellogenin synthesis is detectable earlier in oestrogen-treated animals than in controls, suggesting that also during thyroxine-induced metamorphosis, competence to respond to oestrogen in liver cells is established before effective concentrations of oestrogen are secreted.

To our knowledge these results are the first demonstration that acquisition of competence for oestrogen-dependent synthesis of vitellogenin by the larval *Xenopus* liver is obligatorily dependent on thyroid hormone. However, it is not yet clear whether this maturative event reflects a direct or an indirect effect of the hormone. As it is generally assumed that

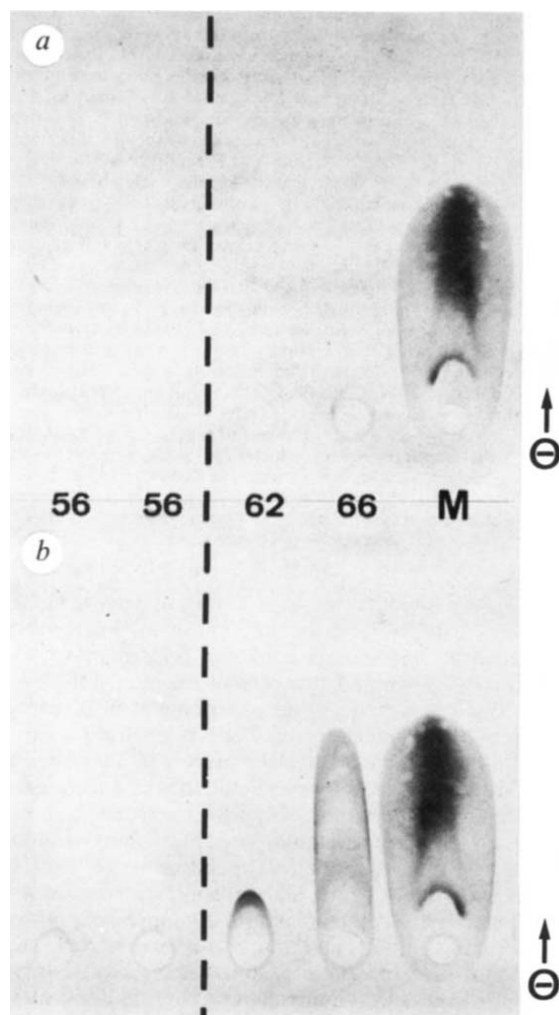


Fig. 2 Detection of vitellogenin in the blood plasma of thyrostatic tadpoles (left) and during thyroxine-induced metamorphosis (right). *a*, No oestrogen; *b*, with oestrogen. Further explanations as for Fig. 1.

Table 2 Absence of response to oestrogen in thyrostatic tadpoles and its induction by L-thyroxine

Thyrostatic tadpoles			
Vitellogenin-positive samples			
Control	Stage; age	56; 45-58	56; 64-68
		56; 75-85	
Oestradiol-3, 17 β		0% (10)	0% (10)
		0% (10)	0% (10)
Thyroxine-induced metamorphosis			
Vitellogenin-positive samples as % of (total)			
Control	Stage; age	60; 85	62; 97
		65/66; 105	
Oestradiol-3, 17 β		0% (5)	0% (5)
		100% (5)	100% (10)

Thyrostatic animals were obtained by raising tadpoles from stage 52 onwards in a solution containing 0.4% thiourea which causes developmental arrest at stage 56 without inhibiting further growth⁸. To induce metamorphosis thyrostatic tadpoles were transferred to 10⁻⁸ M thyroxine (sodium salt, Fluka), which causes a slightly delayed but otherwise normal metamorphosis. Other details as for Table 1.

the effects of steroid hormones are mediated by cytoplasmic and nuclear binding proteins^{9,10}, the larval *Xenopus* liver because of its hormone-dependent maturation of oestrogen sensitivity, provides an ideal model for study of the molecular basis of competence and the primary events of the interaction between oestrogens and target cells.

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Note added in proof: After this paper had been submitted, a similar study was published by Knowland¹¹ indicating that liver explants of *Xenopus* larvae can be induced *in vitro* to synthesise vitellogenin as early as stage 57, but that the appearance of responsiveness is not influenced by oestrogen.

SUSAN HUBER
G. U. RYFFEL
R. WEBER

Zoological Institute,
Division of Cell and Developmental Biology,
University of Bern, Sahlstrasse 8,
CH-3012 Bern, Switzerland

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Identification and change of collagen types in differentiating myoblasts and developing chick muscle

COLLAGEN is the major structural protein in the supporting and connective tissues. In addition to its mechanical properties it has been considered to be involved in developmental processes¹⁻³. Previous studies on muscle development have concentrated on the contractile components but more recently there has been a realisation of the significance of connective tissue in this process. For example, *in vitro* studies have demonstrated the importance of a collagen substrate in promoting myoblast

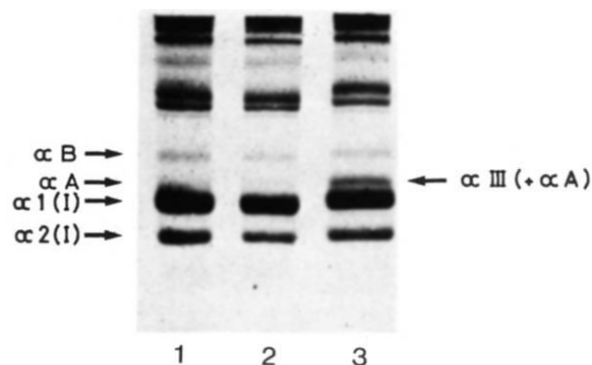


Fig. 1 SDS-polyacrylamide gel electrophoresis patterns showing the presence of type I, type III and type V collagen in 1 M NaCl precipitate from pepsin-solubilised intramuscular collagen from 1-week-old chick limb. Track 1 depicts type I (ratio $\alpha 1/\alpha 2 = 2.2$) and type V collagen; track 2 shows an increase in the $\alpha 1(I)$ band (ratio $\alpha 1/\alpha 2 = 2.8$) due to the presence of $\alpha(III)$ following incubation with dithiothreitol; track 3 shows resolution of $\alpha 1(I)$ and $\alpha(III)$ using a modified interrupted electrophoresis technique¹⁹.

differentiation⁴, and more recently it was shown that all types of collagen were equally effective⁵: Mayne *et al.*⁶ demonstrated that clones of chick muscle cells were capable of synthesising type I collagen. Ketley *et al.*⁵ confirmed the synthesis of type I collagen and also reported that the collagen in intact chick muscle was solely of this type. On the other hand, Duance *et al.*⁷ and Bailey and Sims⁸ investigated the distribution of the genetically different collagen types within bovine muscle connective tissue and showed by biochemical and immunofluorescent techniques a specific distribution of the isomorphous forms of collagen in this tissue. Here we report our studies on the development of chick skeletal muscle which were designed to investigate some pertinent points: (1) the distribution of collagen types in chick muscle; (2) the role of collagen in the development of muscle as a functional tissue; (3) the cellular derivation of these different collagens. Chick muscle was chosen, first, because much is known about its development and differentiation *in vitro*, and second, because it is possible to manipulate its early development in the embryo *in vivo*^{9,10}. We have isolated three isomorphous forms of collagen from chick muscle, and using immunofluorescent techniques demonstrate a specific distribution of these collagen types within this tissue. Finally we have used *in vitro* muscle cell culture to examine how this distribution is achieved by temporal transitions in synthesis. These results suggest that collagen has a subtle role in the development of muscle.

Seven-day-old chick leg muscle was homogenised and the myofibrillar protein was removed by extensive washing. The insoluble intramuscular collagen was digested with pepsin and the solubilised collagens separated by fractional salt precipitation as described previously^{7,8}. Identification of the collagen types was achieved by SDS-polyacrylamide gel electrophoresis (PAGE). Antibodies to the collagen types I and III were raised in New Zealand White rabbits following injection at fortnightly intervals in Freund's adjuvant. Type-specific antibodies were obtained using immunoabsorbent columns to remove nonspecific antibodies as described previously⁷. The sections cut from the leg muscles of chicks were stained with anti-type I or III chick collagen, followed by fluorescein-conjugated anti-rabbit IgG. Rabbit anti-human type V antibodies were found to cross react with chick type V and were therefore used in this study. (We use the term type V to refer to the new type of collagen described by Burgeson *et al.*¹¹ which consists of αA and αB chains, although the molecular compositions as $[\alpha B]_2\alpha A$ (ref. 12) or $[\alpha B]_3$ and $[\alpha A]_3$ (ref. 13) have not been resolved.)

Myoblasts obtained from the thigh muscles of 13-d chick embryos were cultured for 10 d at 37°C on collagen-coated (rat tail tendon collagen) Sterilin dishes. The synthesis of collagen was demonstrated by the incorporation of ³H-proline

Table 1 Detection of collagen types by haemagglutination titres

Day	0	1	2	3	4	5	6, 7	8	9, 10
Type I collagen									
Haem titre	0	0	0	0	1/2	1/2	1/4	1/8	1/8
Type III collagen									
Haem titre	0	0	0	1/2	1/2	1/4	1/16	1/6	1/16
Type V collagen									
Haem titre	0	1/32	1/32	1/64	1/32	1/64	1/512	1/128	1/128

Myoblast culture: 13-d embryo thigh muscle was dissociated using 0.25% trypsin in Earle's balanced salt solution (Flow) for 40 min at 37 °C. After centrifugation the pellet was resuspended in minimal essential medium supplemented with 10% horse serum, 5% chick embryo extract, 500 IU ml⁻¹ penicillin, 500 µg ml⁻¹ streptomycin (Flow), and 50 µg ml⁻¹ ascorbic acid (BDH). Fibroblast contamination was minimised by the 'selective settling' procedure of Yaffe¹⁷ using a type I collagen (rat-tail tendon) coated vessel and incubating at 37 °C for 40 min. Cells were plated at 9.0 × 10⁵ cells ml⁻¹ on 60-mm Nunclon dishes (3 ml) or 50-mm type I collagen (rat tail tendon) coated Sterilin dishes (2 ml) and incubated at 37 °C in a 5% CO₂/95% air atmosphere. The culture medium was replaced every 24 h. Haemagglutination: the culture medium was diluted 1:5 with phosphate-buffered saline and bound to tannic acid-treated sheep red blood cells by the method of Herbert¹⁸. Types I, III and V collagen coated cells were similarly prepared using 10 µg collagen per ml 3% cells. 50 µl of a 2% suspension of coated cells was incubated at 4 °C overnight with anti-type I, III and V collagen. Uncoated cells were used as controls.

into the medium and the determination of ³H-hydroxyproline on the amino acid analyser. The radioactive collagens isolated from the cultures following pepsin treatment were analysed for collagen types by SDS-PAGE. Analysis for the presence of the different types of collagen was also carried out by both the passive haemagglutination and haemagglutination-inhibition techniques.

In these studies we have established that chick muscle is similar to human and bovine muscle in containing collagen types I, III and V (Fig. 1). Immunofluorescence localisation studies have demonstrated the presence of type I mainly in epi- and perimysium, type III in the perimysium and type V in the endomysium (Fig. 2).

The initial studies on cultures of myoblasts *in vitro* using a ³H-proline label demonstrated the synthesis of collagen and that the majority of the collagen was in the medium, in agreement with studies by Lipton¹⁴. Analysis by SDS-PAGE demonstrated the presence of type I collagen but the ³H-activities of the type III and type V collagen bands on the gel were too low to confirm their presence. However, by using the more sensitive haemagglutination and haemagglutination-inhibition assays we were able to demonstrate the presence of all three types of collagen in the medium. The myoblasts fused to form myotubes on the fourth or fifth day in culture. By sequential analysis from day 1, we were able to demonstrate temporal transitions in the nature of the collagen in the medium (Table 1). Thus type I and type III collagen could not be detected until after myoblast fusion, whereas the type V collagen appeared during growth of the myoblasts before myotube formation. This suggests that the presence of type V is probably involved in the sequence of events leading to myoblast differentiation and could serve as a marker of differentiating myoblasts.

Similar temporal transitions of the different forms of collagen have also been observed during normal development *in vivo* at the electron microscope level. Thus, patchy areas of basement membrane appear round the myotubes when they first form in chick limbs after 6 d of incubation, but organised bundles of collagen fibres do not appear in the epi- or perimysium until after approximately 10 d of incubation¹⁰.

The cellular derivation of the different collagen types is not clear. *In vivo* the collagen could be derived from a variety of cells, and even *in vitro* the contribution from a small proportion of contaminating fibroblasts cannot be discounted. Lipton¹⁴ used several rigorous procedures to produce pure myogenic cultures and observed in the electron microscope a patchy basement membrane around myofibres only when fibroblasts

were present, although fibroblasts on their own were incapable of forming basement membrane. Therefore, either the myotubes can proliferate their own basement membrane sheath, or, as the studies of Lipton suggest, a fibroblast-myotube interaction is necessary. This interaction deserves closer examination in view of its possible role in the early development of muscle where a similar interaction between muscle and non-muscle tissue has a major role in the formation of the pattern of muscles^{10,15}. Although it has been suggested that pattern formation could be controlled by diffusible molecules¹⁶, collagen may also have a regulatory role.

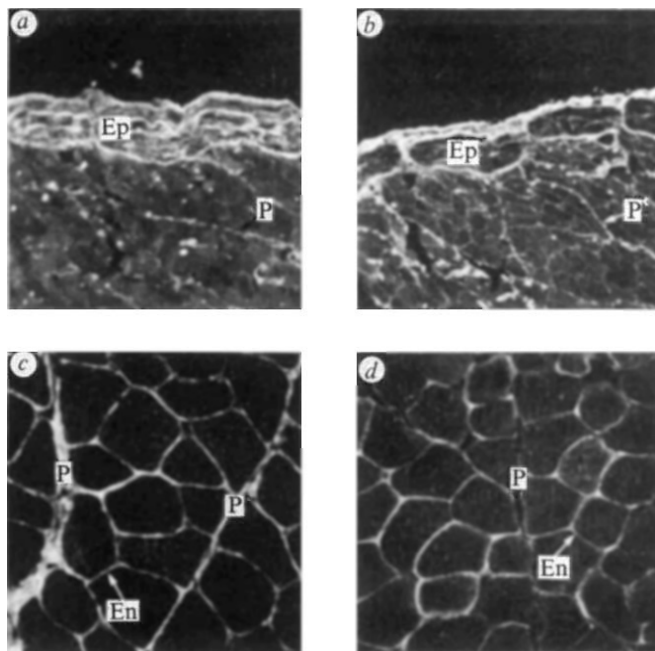


Fig. 2 Immunofluorescent staining of longitudinal sections of chick muscle with anti-type I (a) and anti-type III (b) (×48). Anti-type I stains the epimysium (Ep) and to a lesser extent the perimysium (P). Anti-type III also stains the epimysium but with a different distribution of the stain from anti-type I. Internal staining of the perimysium and slight staining of the endomysium (En) is also observed with the anti-type III. Transverse sections of the chick muscle stained with anti-type III (c) and anti-type V (d) (×96). Anti-type III stains predominantly the perimysium and to a lesser extent the endomysium. Anti-type V staining is confined to the endomysium.

We have demonstrated a spatial distribution of the isomorphic forms of collagen in chick muscle. Furthermore, we have shown temporal transitions of these isomorphic forms *in vivo* and *in vitro*. These studies strongly suggest that there is a role for collagen in muscle development. Therefore we feel that a more detailed investigation of muscle-collagen interactions should now be undertaken to elucidate the precise role of collagen in muscle development. We are now using specific collagen antibodies to study the spatial distribution and temporal synthesis of the collagen types in embryonic muscle *in vivo*.

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A. J. BAILEY
G. B. SHELLSWELL
V. C. DUANCE

Agricultural Research Council,
Meat Research Institute,
and
Department of Animal Husbandry,
University of Bristol,
Langford, Bristol, UK

Received 28 September 1978; accepted 8 January 1979.

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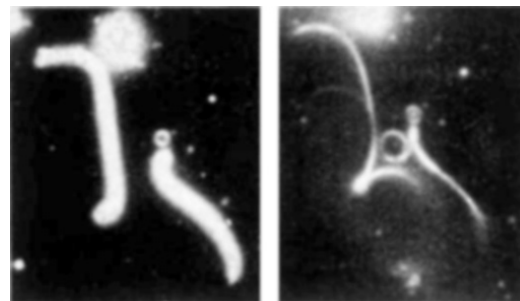


Fig. 1 Dark field images of trypsin-treated ciliary axonemes: left, before ATP addition; right, after disintegration in medium without added Ca^{2+} . $\times 2,900$.

Calcium does not inhibit active sliding of microtubules from mussel gill cilia

CALCIUM ions are probably a general regulator of the assembly state and function of most cytoplasmic microtubules, the organelles upon which processes such as cell division, transport and secretion, as well as ciliary motility, may finally depend. Ciliary activity is the best model we have of microtubule function and the cilium is the only microtubule-based system allowing extensive systematic dissection at present. We are therefore interested in the mechanism by which increasing cytoplasmic Ca^{2+} causes changes in ciliary beat parameters, particularly an arrest response in the mussel (*Elliptio* or *Mytilus*) gill^{1,2}. It is well known that the beat in cilia is the result of a sliding interaction between doublet microtubules in the axoneme^{3,4}, as can be demonstrated directly when axonemes are treated briefly with trypsin⁵. On addition of ATP the trypsin-treated axonemes no longer reactivate to beat normally; instead, they telescope apart. We demonstrate here that, although all sliding of the ciliary microtubules stops during ciliary arrest *in vivo*, the sliding interaction seen after trypsin treatment is not directly inhibited by Ca^{2+} .

The response of intact mussel gill epithelium to increasing cytoplasmic Ca^{2+} can be reproduced experimentally with isolated gill cells and also with detergent-treated models and groups of ciliary axonemes ('eyelashes')^{6,7}. We are able to identify eyelashes that originate from three ciliated cell types: lateral (L), latero-frontal (LF) and frontal (F) cells. When ATP and Mg^{2+} are added, such cell models and axonemes reactivate at about 50% efficiency. We have demonstrated previously⁶ that at 10^{-2} M, Ca^{2+} inhibits 97% of all such motility, that 50% inhibition occurs at about 10^{-4} M Ca^{2+} , and that the L-cilia are differentially inhibited at 10^{-6} M Ca^{2+} .

In this study, cilia from *Elliptio* and related species were mechanically detached from the epithelium and from isolated cells. This suspension was filtered several times through cheese-cloth and the cilia were separated from the cells in a clinical centrifuge at 600g. Cilia were then pelleted in a Sorvall HB4 rotor at 10,000g and resuspended in 1% Triton X-100 in extraction solution (30 mM HEPES, 50 mM KCl, 2 mM EDTA, pH 7.0) for 10 min. They were sedimented again, resuspended in wash solution (30 mM HEPES, 50 mM KCl) and stored on ice for further use. Axonemal protein was determined by the method of Schaffner and Weismann⁸ using bovine serum albumin as a standard. Trypsin digestion was carried out at room

temperature; the trypsin:protein ratio was always $\sim 1:500$. Digestion was stopped after 3 min with excess soybean trypsin inhibitor. Disintegration was followed in a drop of the solution containing trypsin-digested axonemes on a glass slide, covered with a cover slip. Reactivation solution (30 mM HEPES, 50 mM KCl, 4 mM MgCl_2 , 2 mM EGTA, 2 mM ATP, pH 7.0) was drawn in from the side by capillary action, while the axonemes were observed under dark field. Axonemes were photographed before and after ATP reached them. This procedure was carried out in the same way when 10^{-2} M CaCl_2 was substituted for EGTA in the reactivation solution.

In dark field, the axonemes resemble long bright sticks of variable length (Fig. 1). When reactivation solution was added to axonemes not treated with trypsin, they reactivated normally and beat occurred over a long period (10 min observed). Disintegration of axonemes was never found in the presence of ATP if they were treated only with detergent. However, trypsin-digested axonemes twitched briefly when the front of the ATP-containing reactivation solution reached them. Then, they either disintegrated into bundles of thinner sticks (banana peel) (Fig. 1) or elongated (Fig. 2). In many cases, the fragments curved as this occurred. Afterwards, no further movement could be observed. Elongation unequivocally represents active sliding of microtubules, but disintegration might also occur if ATP plasticises the axoneme to permit passive unrolling. Both effects can occur on one axonemal fragment. Figure 2 is the first direct dark field demonstration of sliding in a trypsin-treated metazoan somatic cell cilium.

By a differential count, we quantitated the percentage of trypsin-treated axonemes which underwent either type of movement in various conditions (Table 1). When reactivation solution without ATP was drawn through the preparation, there was almost no effect, whereas about 40% of the axonemes disintegrated or elongated in the presence of 2 mM ATP. It is difficult to separate accurately disintegration and elongation but we estimate that roughly 10% of the axonemes elongate; this varies greatly between preparations. Before addition of ATP, the longer axonemes in the field are approximately the length of L-cilia, that is 14 μm . However, it is not yet possible to determine precisely the origin of the isolated axonemes. To investigate the effect of Ca^{2+} on the sliding in these preparations, 10^{-2} M Ca^{2+} was used in the reactivation medium. The overall extent of disintegration and elongation in the preparation was

Table 1 Effect of calcium addition on amount of axonemal disintegration and sliding

	% (Disintegration plus sliding) $\pm$ s.d.		
	No ATP	ATP alone	ATP, Ca^{2+}
Mussel 1	5 $\pm$ 2.0	43 $\pm$ 10	39 $\pm$ 7.1
Mussel 2	3 $\pm$ 1.0	42 $\pm$ 8	38 $\pm$ 8.1
Average*	6 $\pm$ 2.0	41 $\pm$ 8	39 $\pm$ 8.0

2 mM Mg^{2+} , ATP alone or with 10 mM Ca^{2+} added to trypsin-treated axonemes.

* 20 Experiments.

unaffected by the presence of high Ca^{2+} (Table 1). No obvious change in the relative amounts or the velocities of either of these processes was detectable. Specifically, elongation of axonemes was not inhibited. Photographic and dark field observation of digested axonemes revealed that neither the form nor the final extent of elongation (Fig. 2) was affected. Similar results were obtained when Ca^{2+} replaced both Mg^{2+} and EGTA in the reactivation solution, but preliminary work indicates that the efficiency of sliding is somewhat reduced when minimal Mg^{2+} is present.

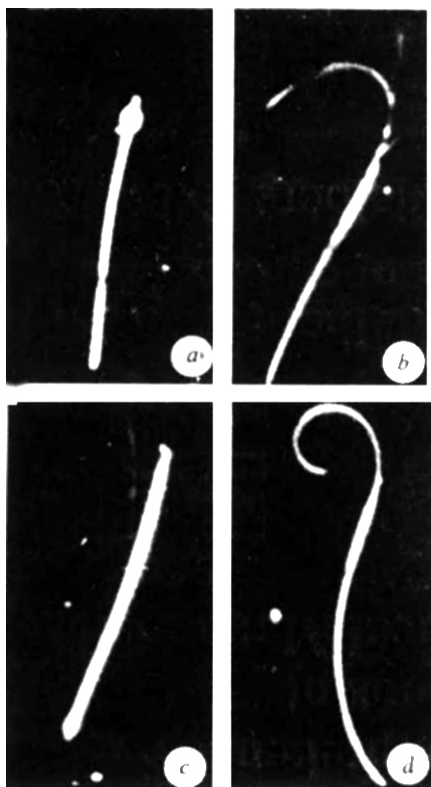


Fig. 2 Dark field images of trypsin-treated ciliary axonemes. Sliding in medium without Ca^{2+} before (a) and after (b) addition of ATP. Sliding in medium with $\sim 10^{-2}$ M Ca^{2+} : c, axonemal length $\sim 13 \mu\text{m}$ before ATP addition; d, after addition of ATP. $\times 3,100$.

These results clarify the effect of Ca^{2+} on the ciliary axonemes. The major bonds that hold the trypsin-treated axoneme together in the absence of ATP are thought to be dynein-tubulin 'rigor' type interactions, described by Gibbons and Gibbons⁹. It is known that ATP will plasticise these bonds, and this may be responsible for the disintegration seen when ATP and Mg^{2+} are added to the trypsin-treated preparations⁴. Because the amount of disintegration seen is relatively unaffected by the presence of high Ca^{2+} , we conclude that Ca^{2+} does not significantly stabilise such bonding. More importantly, Ca^{2+} , at least in concentrations approaching 10^{-2} M, does not affect form or extent of the sliding elongation seen in the trypsin-treated axonemes in the presence of ATP and Mg^{2+} . Also, the overall number of axonemes which slide in the absence compared with the presence of Ca^{2+} does not seem to change. These results make it unlikely that a small subset of doublets or arms in each axoneme is specifically inhibited by Ca^{2+} , although we cannot yet exclude this possibility. In any event, a significant proportion of mussel gill axonemes which do elongate in the presence of Ca^{2+} are likely to be L-cilia axonemes which implies that sliding is little affected by raising the free Ca^{2+} about four orders of magnitude above the concentrations at which it arrests beat. We conclude that the trypsin-resistant dynein-tubulin interactions responsible for microtubule sliding are insensitive to changes in Ca^{2+} that physiologically account for ciliary arrest.

Summers and Gibbons⁵ originally demonstrated that in trypsin-treated sea urchin sperm axonemes, reactivation with Ca^{2+} and ATP would produce sliding, but less effectively than Mg^{2+} and ATP. In *Tetrahymena*, in experiments similar to ours, Sale¹⁰ has shown that trypsin-treated axonemes slide in the presence of 1 mM Ca^{2+} and Mg^{2+} . Zanetti *et al.* (personal communication) have shown that sliding also occurs in *Tetrahymena* when $\text{Ca}^{2+} < 10^{-3}$ M is substituted for Mg^{2+} . Therefore, although these experiments are not as stringent as ours, as in life Ca^{2+} does not cause ciliary arrest in these systems, they essentially confirm our results that in trypsin-treated axonemes high Ca^{2+} has little or no effect on sliding. This is perhaps not surprising as the absence of complete arrest as a physiological effect of increasing cytoplasmic Ca^{2+} implies continued microtubule sliding. For example, in *Paramecium*, increasing intracellular Ca^{2+} to greater than 10^{-6} M causes ciliary reversal with continued beat¹¹. In *Tetrahymena*, despite such possible ciliary reversal, the direction of active sliding is unaffected by increased Ca^{2+} (ref. 10).

We also conclude that the Ca^{2+} -sensitive system responsible for ciliary arrest originally present in isolated gill cilia is destroyed by brief treatment with trypsin. Concomitantly, there is a change in ciliary behaviour such that the normal constraints on microtubule sliding disappear and the axoneme telescopes apart rather than beating. It is possible that these two effects are correlated. Any or all of three morphological components of the axoneme might be implicated in either Ca^{2+} -sensitive or Ca^{2+} -insensitive aspects of the normal control of sliding: the dynein arms themselves; the spoke, central sheath complex; and the interdoublet (nexin) links. Spokes and interdoublet links are degraded by trypsin¹², although spokes at least do not completely disappear⁴. Despite the relative stability of the dynein arms, and their demonstrable function after trypsin digestion, some regulatory component of these structures might also be degraded. Detailed study of the biochemical effects of trypsin treatment has not been undertaken and might prove profitable. Of these components, the spoke-central sheath interaction has been most directly implicated in the control of sliding, both on morphological grounds by Warner and Satir¹³, and in mutant studies¹⁴. The detailed interaction of this system with Ca^{2+} is not understood. Also, the precise mechanisms by which sliding is converted into bending and otherwise restricted or synchronised in the intact axoneme in $\text{Ca}^{2+} < 10^{-7}$ M are still unknown. When these features are more accurately specified, it should be possible to localise the site(s) of interaction of Ca^{2+} and to detect how appropriate changes in axonemal behaviour are produced by increasing axonemal Ca^{2+} .

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MARIKA F. WALTER*
PETER SATIR

Albert Einstein College of Medicine,
Bronx, New York 10461

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* Present address: Department of Zoology, University of Tübingen, FRG.

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Effect of modification by *N*-acetoxy-*N*-2-acetylaminofluorene on the level of DNA methylation

DNA METHYLATION in prokaryotes as well as in eukaryotes is a widely distributed phenomenon but its biological role is still unknown. In prokaryotes, DNA methylation has been related to restriction recognition mechanisms^{1,2}; no such correlation has yet been found in eukaryotes. Some investigators have suggested that cell differentiation and embryogenesis might be related to DNA methylation³⁻⁵. The recent results obtained by Christman *et al.*⁶ indicating that inhibition of DNA methylation may promote the differentiation and expression of globin genes in Friend erythroleukaemia cells, support this theory. In their observations, inhibition of DNA methylation was obtained after treatment of the cells with the methionine analogue L-ethionine which is also a carcinogen. We have used another approach where the change of levels of *in vitro* 5-methyl-³H-cytosine formation in DNA following DNA modification with a carcinogen is observed. We have chosen *N*-acetoxy-*N*-2-acetylaminofluorene (*N*-AcO-AAF) which reacts covalently *in vitro* and *in vivo* with nucleic acids and proteins⁷⁻⁹ and which, after administration to animals, induces tumours¹⁰.

The DNAs substituted mainly in position 8 of guanine moieties by this aromatic compound were used as substrates for DNA (cytosine-5) methyltransferase from rat brain tissue, an organ which exhibits high levels of methyltransferases during early postnatal development (ref. 11 and unpublished results), and has been compared to non-substituted DNA for the methyl acceptance capacity.

Table I The methylation of DNA modified by *N*-AcO-AAF or *N*-AcO-AAIF

Substrate	% Methylation*
Non-modified DNA	100.0
DNA-AAF; 4.0%	46.8 ± 5%
DNA-AAIF; 4.5%	37.0 ± 2.4%
DNA-AAIF; 5.8%	32.2 ± 2.2%

Unmodified or carcinogen-modified chicken erythrocyte DNAs (40 µg) were obtained as described in the legend to Fig. 2. DNA from chicken erythrocyte was modified by *N*-AcO-AAIF following the procedure previously described¹⁸. DNA substrate was incubated with DNA methyltransferase from rat brain in conditions as described in the legend to Fig. 1.

* Percentages of methylation were calculated using unmodified chicken erythrocyte DNA as control (100%). Values are from experiments made in triplicate.

The optimal conditions for the methylation of DNA by *S*-adenosyl-L-methyl ³H-methionine in the presence of rat brain DNA (cytosine-5) methyltransferase, using DNAs from chicken erythrocyte or *Micrococcus lysodeikticus* (Sigma), were first established. The substrate concentration which gave a maximal reaction velocity with 0.6 mg of brain enzyme protein was 40 µg of DNA. At this DNA concentration the maximal level of methylation was obtained after ~90 min of incubation (Fig. 1). 5-Methyl-³H-cytosine was the only methylated base found to be present in DNA reacted with the brain enzyme (data not shown).

DNAs from chicken erythrocyte or *Micrococcus lysodeikticus* were then reacted with *N*-AcO-AAF resulting in the production of carcinogen covalently bound to the guanine residue of DNA¹². By using the appropriate conditions DNA could be modified *in vitro* by the carcinogen to a varying extent. Calculations to establish the percentage of modified bases have been described elsewhere^{13,14}.

Figure 2 shows the results obtained if carcinogen-modified DNAs (DNA-AAF) were methylated in the presence of rat

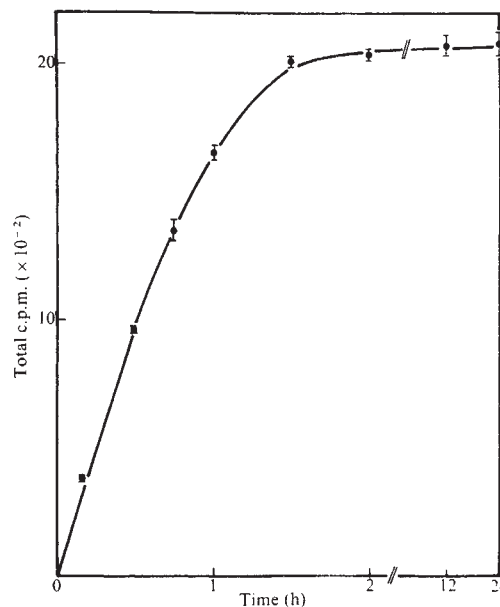


Fig. 1 Methylation of chicken erythrocyte DNA by rat brain DNA (cytosine-5) methyltransferase. The standard assay mixture contained in a final vol. of 0.2 ml: 40 µg DNA, 0.35 µg pancreatic RNase, 0.4–0.6 mg enzyme protein, 2 µCi *S*-adenosyl-L-methyl-³H-methionine, 50 mM Tris-HCl pH 7.8 and 0.5 mM dithiothreitol. After incubation at 37°C for various times 0.6 ml H₂O and 1 ml of carrier calf thymus DNA solution (1-mg ml⁻¹) were added to the incubation mixture. This was then deproteinised with SDS sodium perchlorate and extracted with a mixture of chloroform-isoamyl alcohol as described elsewhere²⁵. The final DNA pellet was dissolved in 0.1 M NaOH (0.2 ml) by incubation at 90°C for 10 min. The clear solution was transferred to a scintillation vial and the radioactivity measured after complete mixing with Unisolve 1, scintillator (Koch-Light). Blank values were subtracted in each case. Averages with s.d. are from two experiments made in duplicate. Brain enzyme was prepared as follows: Sprague-Dawley rats (7-day-old) were decapitated and the brain cortices quickly removed and homogenised with buffer solution containing 2.4 M sucrose and 0.001 M MgCl₂ in a glass-Teflon Potter-Elvehjem homogeniser to a final volume of 10 times the tissue weight. The remaining procedure was similar to that described by Pogo *et al.*²⁶. All operations were carried out at 4°C unless otherwise stated. Protein concentration was determined according to Lowry *et al.*²⁷ using bovine serum albumin as standard. Chicken erythrocyte DNA was obtained using the procedure described elsewhere²⁸.

brain DNA methyltransferase. An inverse relationship between levels of DNA methylation and the percentage of modified bases residues in the DNA structure was observed. The inhibition of DNA methylation cannot be attributed to the presence of free carcinogen in the reaction mixture as any unreacted carcinogen was removed from the DNA solution.

Similar results obtained using carcinogen-modified DNAs from two sources having a different GC content showed that inhibition of DNA methylation is independent of the nature of DNA used.

We also modified DNA with *N*-AcO-AAIF (DNA-AAIF) which is the 7-iodo derivative of *N*-AcO-AAF. Fuchs *et al.*¹⁵⁻¹⁸ reported that in DNA-AAIF the iodo fluorene ring lies along the phosphate-sugar backbone of the double helix (outside binding model) whereas in DNA-AAF the fluorene ring is inserted between the adjacent base pairs (insertion-denaturation model). Therefore, we tested DNA-AAIF to see whether the different positioning of the fluorene residue could diversely affect the methyl accepting properties of nucleic acid. As shown in Table I, the iodo derivative substituted DNA behaved in a way similar to DNA-AAF, showing that different positioning of the fluorene ring within the DNA molecule does not change its inhibitory properties.

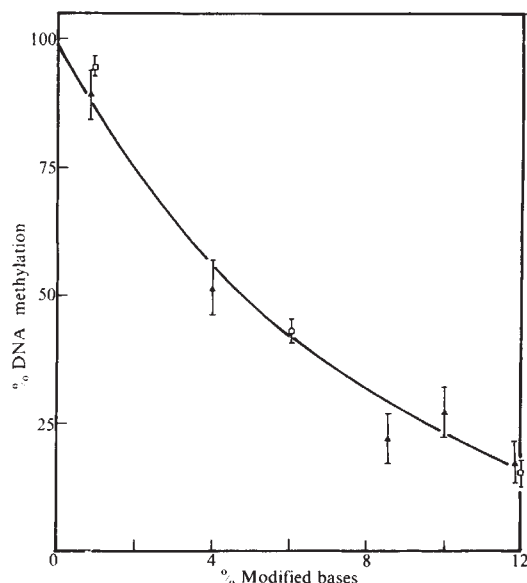


Fig. 2 The methylation of *in vitro* modified DNA-AAF: *N*-AcO-AAF was synthesised by a modification of the original procedure²⁹ consisting of direct hydrogenation and acetylation of 2-nitrofluorene¹⁸. The purity of the *N*-AcO-AAF obtained was checked by thin-layer chromatography and the final product was stored at -20°C as alcoholic solution. DNA bound to the carcinogen was prepared as described elsewhere¹⁴. The *in vitro* methylation reaction of carcinogen modified chicken erythrocyte or *Micrococcus lysodeikticus* DNAs were done as described in the legends to Fig. 1. The results are expressed as percentage relative to control (100%) determined with non-modified DNA. Blank values were subtracted in each case. $\blacktriangle$, Chicken erythrocyte DNA; $\square$, *Micrococcus lysodeikticus* DNA. Averages with s.d. are from four experiments made in triplicate except for the experiments with 11, 3 and 13% modified bases, which are in duplicate.

Arylamidation of the C8 of guanine by some aromatic amines induces a rotation of the bases involved in the double helix, altering its normal conformation and carrying a local destabilisation of the DNA^{13,19}. As shown previously, this local denaturation may change some biological properties of nucleic acids by inducing cell transformation¹⁰, premature termination of transcription at (or near) the site of DNA modification²⁰ and more recently *in vitro* inhibition of DNA synthesis²¹. Our results show that modification of guanine residues in position 8 impairs another biological property of the double helix.

The carcinogen mediated effect on DNA methylation can be explained as follows: binding of methyltransferase near or at the position of methylation in DNA may be hampered by the presence of carcinogen-modified guanines paired to cytosine residues susceptible to methylation. Consequently, DNA methylation is now restricted to cytosine residues paired to guanines which are recognised by the enzyme but which are not modified. Alternatively, it can be proposed that binding of the enzyme to DNA is not affected by the presence of carcinogen, but, subsequent walking of the enzyme on the DNA helix is interrupted by the carcinogen. The last alternative agrees with the proposed mechanism of action described for DNA (cytosine-5) methyltransferase²²⁻²⁴ as well as with the model explaining the premature termination of DNA transcription in DNA-AAF²⁰.

Further work is required to establish whether DNA under-methylation caused by aromatic amides *in vitro* can also be obtained *in vivo*, and to demonstrate the effect of DNA hypomethylation on cell differentiation and/or carcinogenesis.

We thank Professor M. Daune for hospitality and criticisms; also Dr R. P. P. Fuchs for discussions, and *N*-AcO-AAIF. This work was supported by the Ligue Nationale Française contre le

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C. E. SALAS

A. PFOHL-LESZKOWICZ*

Laboratoire de Biochimie

M. C. LANG

Laboratoire de Biophysique

G. DIRHEIMER*

Laboratoire de Biochimie,
Institut de Biologie Moléculaire et Cellulaire du CNRS,
15 rue Descartes,
67000 Strasbourg, France

Received 5 October 1978; accepted 19 January 1979.

*Also at: Faculté de Pharmacie, Université Louis Pasteur, 3 rue de l'Argonne, Strasbourg, France.

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Corrigenda

In the letter 'Speciation of dissolved iodine in estuarine waters' by J. D. Smith and E. C. V. Butler, *Nature* **277**, 468-469, the values for iodine in the Yarra River estuary in the text and Fig. 1 should all be multiplied by 0.73.

In the letter 'Opposing effects of tumour promoters on erythroid differentiation' by R. M. Miao *et al.*, *Nature* **274**, 271 (1978), all concentrations of 12-*O*-tetradecanoyl-13-acetate (TPA) should be 1,000-fold lower than stated; for example, 10^{-7} M should read 10^{-10} M.

Errata

In the letter 'Immunological properties of the surface of parasitic nematodes' by C. D. Mackenzie, P. M. Preston and B. M. Ogilvie, *Nature* **276**, 826-828 (1978), the following acknowledgement was omitted: 'P. M. Preston was financed by a grant from the World Bank/UNDP/WHO Special Programme for Research and Training in Tropical Diseases.'

In the letter 'Measurement of carbon tetrafluoride in the atmosphere' by R. A. Rasmussen *et al.* *Nature* **277**, 549-551, for % read p.p.t. (parts per 10^{12}), and in paragraph 8, line 8, for p.p.b. read p.p.t.

STUDENT BOOKS SUPPLEMENT

By the time the ink has dried

David W. Hughes

WHAT use is a textbook? The answer to this question depends to an extent on which side of the desk you are sitting. Cynically we could say that the main aim of the student is simply to pass the final examination. He judges the worth of the recommended textbook by its ability to help him achieve this aim with the minimum of effort. The book should contain the course material to the depth required and hopefully little other additional material and too much rigour leading to confusion. There should be a reasonable selection of examination-type questions at the end of each chapter plus answers and wherever possible worked solutions.

The lecturer approaches the book from a different standpoint. He passed all the exams a long time ago. For him the book should fill in the background to the subject, giving it roots so to speak. It should be a guide to his lectures so that the student can turn to it to clarify the difficult or unclear bits. Also it should be a stepping stone for the mind, leading the student on to more detailed studies and to the boundaries of the subject, the regions which are uncertain, the outstanding problems, the next steps in the pursuit of knowledge and understanding.

Starting near the intellectual top end of the market we have the second edition of *The New Cosmos* by Albrecht Unsöld (Springer: New York and Berlin; paperback DM37, \$18.50). This is an English translation of the 1974 German work. The book has two main audiences, the first being the final-year astronomy and astrophysics student who is looking for an overview of his subject. The fact that the book only has 451 pages is an enormous advantage here as it saves the student, especially near exam time, from the task of wading through the larger monographs. It is also an excellent kick-off point for the mathematics or physics graduate who has turned to astronomy, astrophysics, space science or cosmology as a research field. It is a comprehensive but concise introduction to these topics. The book has an abundance of clear figures and takes pains to stress the delicate balance between the advances in observational work and in theoretical studies. It is geared to the numerate student and does not shirk the responsibility of tackling properly such subjects as radiation theory, stellar interiors, galactic rotation, degenerate matter and galactic and stellar evolu-

tion. Many astronomers and astrophysicists today grew up in their subjects clutching the first edition. This new edition is for the next generation and it can be classified simply as essential reading, a Godsend for student, lecturer and research worker alike.

Moving down the scale of complexity we come to *University Astronomy*, by J. M. Pasachoff and M. L. Kutner (W. B. Saunders: Philadelphia and London; £13.50), a book which includes basic algebraic and trigonometric equations but excludes infinitesimal calculus. The authors are aiming at the science student during his first year at university and they are well on target. The book describes our modern knowledge of astronomy, fitting the latest discoveries alongside the established results. It is refreshing to notice how the authors discuss the whole spectral range of observations as they go along and don't for example relegate all the radio work to a separate chapter. It was a book full of surprises, the large majority of them pleasant ones. It was heartening to come across time and again examples of the authors' desire to stretch the student by introducing him to the problems of experimentation and theorising. The book contained many thought-provoking sections, a few examples taken randomly includes a discussion of the errors of observation and standard deviation, Occam's Razor, the futility of studying UFOs and a Martian's investigation of life on Earth. The authors don't turn their backs on the basic physics which underlies many of the facets of astronomy but explain physical concepts in a clear and concise way. Each chapter is rounded off by a summary and outline section, a collection of multiple choice review questions, discussion topics and suggestions for additional reading. The authors are to be congratulated on producing such an excellent résumé of the subject and one that, considering the self-imposed mathematical limitations, never deviates from stressing the underlying scientific principles of astronomy.

J. M. Pasachoff and M. L. Kutner have very thoughtfully produced a *Teacher's Guide to University Astronomy* (W. B. Saunders: Philadelphia and London). This contains the answers to all the questions posed in their main textbook (considering my initial comments a more appropriate title might have been *Student's Guide*). It also has a set of seven

laboratory exercises together with the addresses of the suppliers of the relevant planetary, solar and sky photographs, some sample exam papers, possible syllabi and lists of audiovisual aids and stockists. It is obviously geared for the American market, but don't let this put off the non-American readers.

Both *University Astronomy* and the next book under review *Astronomy Now* (by J. M. Pasachoff; W. B. Saunders: Philadelphia and London; \$11.95) come from the same stable. *University Astronomy* contains more about the Earth and relativity, and does use basic mathematical equations. *Astronomy Now* is a shorter non-mathematical version, ideal for the innumerate and the mathematicophobes. It is difficult to advise you which one of these books to choose. I know that a student who does not need the algebra would automatically pick the book without it and thus avoid the confusion. But I like mathematics and physics; they add spice to astronomy and I would advise the students to buy *University Astronomy* in the hope that some of the joys of maths and physics rubbed off.

This review continues by considering four, more descriptive astronomy textbooks which have been tailored to the needs of the American university humanities and social science students who have usually to take two one-year courses (each of about 72 lectures and lasting two semesters) in natural science subjects. I need not go into details as to why astronomy should be and is in many cases an obvious choice. Many books are produced at the required level and I have often felt apprehensive about this market being flooded. But this worry is groundless because the market is enormous. R. H. Garstang (*J. Br. Astr. Assoc.* **88**, 407; 1978) gives two examples. Of the 20,000 students at the University of Colorado, 1,050 are taking the astronomy course. If we select another university more or less at random we find that 2,000 students at the University of Texas are also doing elementary astronomy. It's little wonder that a fair armful of these books are published each year considering that the publishers break even point is in the low thousands.

I am confronted with *Discovering Astronomy*, by Robert D. Chapman (Freeman: San Francisco; hardback £12.60, paperback £7), *The Restless*

Universe: An Introduction to Astronomy, by Harry L. Shipman (Houghton Mifflin: Boston and London) and *Cosmic Evolution: An Introduction to Astronomy*, by G. B. Field, G. L. Verschuur and C. Ponnamperuma (Houghton Mifflin: Boston and London). They are all the same size; one is red, the other blue, the third brown; they contain the regulation collection of colour plates (where would one be without NASA and the Hale Observatory?). They are beautifully illustrated, well indexed, glossaried and spattered with review questions, thought questions, chapter summaries, and the like. They each have their 'special way of approaching the subject'. Chapman uses the historical angle starting from Earth and the view Earth dwellers have of the sky and moving out to the Moon, Sun, planets, stars, Galaxy and Universe. Shipman states that he differs from similar texts by stressing contemporary astronomy—"the events that readers have probably heard about from the media" and by indulging in a detailed inter-comparison of the 'geological' processes that have taken place on different planets. Field, Verschuur and Ponnamperuma concentrate on the evolution of the Universe from the primordial big bang, through the origin of galaxies, stars and planets to the emergence of intelligent life on Earth and the search for this life elsewhere. Even with these 'different approaches' in mind I was continually struck by the overall similarities. The differences are at best marginal and in most cases simply seem to be a difference in the ordering, much more than in content. In my experience it is a very rare lecture course that starts at the beginning of a textbook and methodically works its way through chapter by chapter, so the ordering is fairly immaterial. So I have a problem, which one should I recommend? I shudder to suggest that you should resort to 'eeny, meeny, miney, mo' but this is one way out. The other is simply to buy the cheapest.

My final book is *Exploration of the Solar System* by William J. Kaufmann III (Macmillan: New York; Collier Macmillan: London; £0.00). A book mainly about planets with a little on stars and a sister to Kaufmann's 1977 book *Astronomy: The Structure of the Universe*, which was mainly about stars with a little on planets. Kaufmann's books are aimed at the same non-mathematical audience as the three mentioned above. In *Exploration of the Solar System* one feels that Kaufmann has room to spread himself. He is not restricted by space, he studies the problems, the spacecraft, the explorations, the data, the planetary features and models in detail and with thoroughness.

In general I was extremely impressed by the high standards of these books. Writing an astronomical textbook is no easy task because topic after topic is in a state of rapid development. To quote

R. D. Chapman "it is inevitable that some parts of any book will be out of date by the time the ink has dried". It must be frustrating to write a line such as "ratio of the mass of Pluto to that of the Earth = 0.1(?)" months before the mass was accurately measured due to the discovery

and observation of a satellite. But still the job needs to be done and it is refreshing to see how well it is done time after time. □

David W. Hughes is Lecturer in Astronomy and Physics at the University of Sheffield, UK.

Mathematical cosmology

Mathematical Cosmology. By P. T. Landsberg and D. A. Evans. Pp. 306. (Clarendon/Oxford University Press: Oxford, 1978.) £6.95.

ALTHOUGH the number of students taking formal courses in cosmology is tiny, most physical scientists find room in their activities to read up on it from time to time. The stumbling block to penetrating the technical literature is usually the general theory of relativity, with its mind bending concepts of curved spacetime, event horizons and singularities, not to mention punishing (as well as non-linear) mathematics. The solution to this barrier is, according to P. T. Landsberg and D. A. Evans in their *Mathematical Cosmology*, a "stage II" book, defined in their introduction to be one which goes beyond the broad qualitative outlines, but uses only simple mathematical arguments. And this means no general relativity. Although other books, such as Michael Berry's *Principles of Cosmology and Gravitation* (Cambridge University Press: Cambridge, 1976), also attempt this, Professors Landsberg and Evans give probably the first comprehensive and systematic non-relativistic treatment of cosmology.

Students familiar with Bondi's little book *Cosmology* will know how fairly elementary Newtonian ideas can lead effortlessly to the main features of relativistic cosmological models. The authors of *Mathematical Cosmology* follow the same path, but in a much more elaborate and detailed

fashion. All the basic conceptual structure is kept simple, and individual chapters can be read in isolation without difficulty. The standard Friedmann models are thoroughly discussed, and even the steady state theory gets a mention. Each chapter is terminated with a set of questions, the answers to which are provided.

Eventually, perhaps inevitably, general relativity refuses to be excluded. It starts to intrude during the discussion of light propagation, and the appearance of distant astronomical objects. However, in this limited context, without the field equations of the dynamics of the space-time itself, there is nothing terribly formidable about it, and students should be able to follow the lucid discussion easily. When horizons are finally discussed, the approach is physical rather than geometrical, and indeed, this is true of the book as a whole.

For scientists and students with a serious interest in cosmology who do not want to invest a lot of time learning differential geometry and topology, *Mathematical Cosmology* gives an excellent, clearly written and coherent account of the standard theory which is very economical on intellectual investment. It is also economical on the pocket, because at £6.95, it is rather cheap for a 306-page textbook. For students taking cosmology the hard way, it is still worthwhile background reading, and of course most of the results are identical to those of the relativistic treatment.

Paul Davies

Paul Davies is Senior Lecturer in the Department of Mathematics at King's College, University of London, UK.

Vibrations and waves

Vibrations and Waves in Physics. By I. G. Main. Pp. 336. (Cambridge University Press: Cambridge and London, 1978.) Hardback £17.50; paperback £4.95.

THIS is an excellent book which can be thoroughly recommended either for use as a text for an undergraduate course introducing the student to many of the widely-pervading notions connected with vibrations and waves, or indeed for anyone who feels the need

to broaden his knowledge in this sphere. The material covered starts with free vibrations and continues through forced vibrations, anharmonic and coupled vibrations, to non-dispersive waves. After a short chapter on Fourier theory the author looks at dispersion, water waves, electromagnetic waves, and de Broglie waves, and finally covers basic ideas in the reflection, refraction, and diffraction of waves.

The book is well and carefully constructed. An example of this is the useful flow diagram near the front, showing clearly which are the parts containing the coherent development

of the basic theory and which are what the author, in his preface, terms "excursions into physics". These "excursions" demonstrate the way in which many useful results and an increased understanding of many physical phenomena may be obtained by simple order-of-magnitude calculations.

One of the best features of the book is the choice of problems at the end of each chapter. These span a wide variety of topics and put the material of the preceding chapter firmly in a physical context. In the penultimate chapter, for example, the problems include such apparently disparate topics as reflection of electromagnetic waves from a plasma, reflection and refraction of shallow water waves at a change in depth, evaluation of lattice heat capacity at low temperatures, and calculation of the Fermi energy for conduction electrons in a metal. Almost every chapter includes at least one ingenious and non-run-of-the-mill problem illustrating the application of previously presented principles in an

everyday situation. Hints for solution are given for some of the problems, and answers are provided to all numerical questions.

The similarity of both the chapter headings and the length of the book invite comparison with Pain's *The Physics of Vibration and Waves* (Wiley: London, 1976). In fact the books are very different in character. Main spending much more time on explanation, whereas Pain covers a wider variety of material with, therefore, much less explanation. The difference between the books is well illustrated by the fact that the summaries that occur at the end of each chapter in Pain are primarily lists of formulae, whereas the summaries at the end of each section in Main are primarily words.

S. J. Flockton

S. J. Flockton is Lecturer in the Applied Acoustics Group, Department of Physics, Chelsea College, University of London, UK.

Classical mechanics

Mechanics and Motion. By L. Mackinnon. Pp. 161. (Clarendon/Oxford University Press: Oxford 1978.) Hardback £6; paperback £3.25.

A SOUND knowledge of classical mechanics is a valuable asset to anyone studying physics, and this book attempts to deal with those branches of the subject which are most relevant to the modern physics in a university degree course. The book is based on a first-year lecture course given by the author at the University of Essex. The basic concepts of mechanics are discussed in the first chapter, after which there are chapters on rotary motion, vibratory motion, wave motion and central force motion. Finally, there is a chapter entitled "Analytical Classical Mechanics" in which the concepts of Lagrangian and Hamiltonian mechanics are introduced.

The amount of material seems to have been limited to what can reasonably be included in a lecture course of approximately 30 one-hour lectures, and within this constraint the choice of material is reasonably satisfactory. However, the level of presentation is sometimes rather superficial. For example, there is no precise definition of the centre of mass of a system of particles, and there is only the briefest mention, and then only for the particular case of the centrifugal force, that the inertial reactions on a body act through the centre of mass. The

concept of reduced mass is not discussed at all, beyond a brief statement in the conclusion to the chapter on central force motion that the source of the force is free to move. Also, the theorems of parallel and perpendicular axes for moments of inertia are quoted without proof, the proof of the parallel axes theorem being left as an exercise for the reader. As a further example in a more advanced topic, the Coriolis force is introduced by considering a very specific problem; and the general form of the force is then stated without proof, as a rigorous treatment of the equations of motion in a rotating frame of reference is not given. Even if such a detailed treatment is considered to be unsuitable in a lecture course, it should be included in a book based on that course. A similar objection can be levelled at the discussion of Fourier analysis in the chapter on vibratory motion.

Apart from these deficiencies the book is quite well written in a style which is rather less formal than that of most books on classical mechanics. Unfortunately the standard of production is lower than one would have expected from the Oxford University Press. The book is printed directly from the type-written manuscript and some of the equations are rather badly composed. This technique may reduce the cost of production slightly, but the result is visually far less attractive than a type-set book.

J. W. Humberston

J. W. Humberston is a Lecturer in Physics at University College, London, UK.

Fluid mechanics experiments

Fluid Mechanics: A Laboratory Course. By M. A. Plint and L. Bosworth. Pp. 186. (Charles Griffin: London and High Wycombe, UK, 1978.) Paperback £5.95.

THIS book describes, in a clear and interesting manner, a series of fluid mechanics experiments using low and high speed air jets, low speed and supersonic wind tunnels and a pipe flow apparatus. The low speed experiments in air include the pressure distribution around a cylinder, the lift on an aerofoil, boundary layer traverses on a flat plate and jet and pipe flow traverses. The compressible flow experiments measure mass flow and pressure distributions in convergent-divergent nozzles and look at the supersonic flow past a double wedge aerofoil. Lubrication properties of an oil wedge using a Michell bearing rig, and the properties of an air bearing are also treated.

In cases where theory depends primarily on the physics, relevant equations are derived; otherwise the necessary equations are quoted. The authors present their own results in detail and explain the reasons for any differences that occur between theory and experiment. They argue quite rightly that details of this type can help students to look critically at their own experimental results even though the apparatus may be different. The experiments are typical of those in undergraduate courses in aeronautical, mechanical and civil engineering and in some physics courses. However, they represent only a selection of the vast number of valuable undergraduate experiments in fluid mechanics and it is unlikely that the experiments at a particular university or polytechnic will coincide exactly with the authors' choice. Even so the book would be helpful as additional reading for undergraduate engineers. The book would be most useful to those who, on entering the teaching profession, require to establish a series of fluid mechanics experiments.

The book is well presented with good diagrams, and it is not in competition with other books. For completeness it could have included more on the estimation of experimental errors but this can be found in other textbooks on experimental techniques.

T. N. Stevenson

T. N. Stevenson is Senior Lecturer in the Department of the Mechanics of Fluids at the University of Manchester, UK.

A detached view of the Earth

Remote Sensing: Principles and Interpretations. By F. F. Sabins, Jr. Pp. 426. (Freeman: San Francisco and Reading, 1978.) \$25; £14.50.

To the casual or unwary would-be customer the title of this large new volume suggests a comprehensive treatment of the explosively expanding modern science, remote sensing of the environment. This is an impression which would not be properly rectified by a reading of the Preface, in which the author confesses that "... because I am a geologist by training and early experience, there is a tendency (in this book) to emphasise the geologic aspects of image interpretation". Nor would it be clear from the nearby statement that "This book is designed for a one-semester or one-quarter course for upper-class or graduate students with no previous training in remote sensing" that the treatment is, in fact, very heavily biased towards the needs of the student of geology, as distinct from his colleagues in environmental science, geography, and allied fields.

To some potential purchasers, this could prove to be a very valuable volume, selecting and synthesising smoothly from the physical bases on which environmental remote sensing rests, by way of the wide variety of means by which different statements of the targets of remote sensors are acquired and analysed, to some of the chief spheres of remote sensing data applications. However, if one's interests are not predominantly concerned with the ways analyses of spacecraft (especially Landsat) data may supplement the longer-established practices of photogeology using images from aircraft and balloons, purchasing this book could be a costly mistake.

My own view is that the confessed "tendency to emphasise ... geologic aspects" is an exceptional understatement, given the breadth of cover suggested by the title. Certainly, the author does not spend all his time discussing geological phenomena, for there are useful sections on water and ice, but other classes of phenomena with which a more broadly-based student would be significantly concerned—including other surface features such as snow, land use and crops, natural vegetation, and settlements, and atmospheric phenomena such as energy budgets, cloud organisations and winds—are treated very cursorily indeed. One notable example concerns applications of remote sensing

techniques in land-use studies: this vitally-important field, full of economic and humanitarian potential, is credited with less than three pages of text. The atmosphere is ignored to such an extent that thermal infrared imagery from meteorological satellites is said to be useful for many applications—but none of those mentioned are atmospheric. Indeed, it is stated that "Clouds and surface winds ... produce confusing patterns" in such images, and that such factors must be considered (as complicating factors) by the

(surface) interpreter. Such a treatment is scarcely fair when the primary purposes of these satellites are recalled.

Had this textbook been entitled *Satellite Remote Sensing for the Geologist*, I would have applauded it; as it is, I cannot do so for, as a teacher in geography, I find the contents much too restricted for general courses in remote sensing of the environment.

Eric C. Barrett

Eric C. Barrett is Reader in Climatology and Remote Sensing at the University of Bristol, UK.

Global geology

Earth. By F. Press and R. Siever. Second edition. Pp. 649. (Freeman: San Francisco and Reading, 1978.) Hardback \$16.95, £9.90; paperback (Europe and Africa only) £7.20.

THIS is an excellent book which is specifically designed as a teaching text, but could equally well be read without supervision. It is intended for students with no previous science background and who may not necessarily be intending to follow a geological career—though this text may well encourage them to do so. The reader is not treated as an imbecile, but is assumed to be a reasonable, intelligent being to be treated as an equal. The care that must have been taken in the wording does not show and, while American in spelling, the language is the best form of English—American.

It is arranged in three basic sections: (1) the Earth as an evolving planet and how it is studied, (2) surface processes and (3) internal processes and their surface effects. The individual chapters are planned to be read in any order, which could be dangerous as such a versatility means that repetitions are inevitable. In this case, however, the repetitions are extremely well done, usually comprising a review from a slightly different angle from the way in which the original version was presented.

Each chapter has its own brief introduction and summary, together with a few exercises and a bibliography. The summaries are too brief to act as 'crib-sheets', but the exercises show a healthy versatility, sometimes requiring calculations to be made but usually calling for a real assessment of some particular problem—unlike exercises in some books that merely require the student to find the appropriate section for regurgitation without further thought.

Some difficulties could arise if the student is not following the actual order of the chapters as occasionally relevant information has appeared in previous chapters. Specific sections within a

chapter are boxed off for more detailed consideration, usually at a slightly higher level than the main text, and it is a pity that the bibliography is not similarly indicated as being at a complementary or more advanced level.

The reviewer's standby of printer's errors appear to be absent and the whole volume is very well produced, usually with two text columns per page (25½ × 21 cm) with a wide margin often utilised for figure legends. The illustrations are well chosen, though almost entirely North American, and plentiful, both in terms of photographs and two-tone diagrams—the brown tone allowing attention to be brought to the more salient points. There is a good glossary and index, which will be essential for the new student or for later reference.

The only minor murmurs must be in the coverage of palaeontology in general and of sedimentary structures. In both cases, discussion of the use of fossils for stratigraphical purposes is surprisingly inadequate for any potential geology student. Detailed consideration of the evolution of a trilobite's eye can well be assigned to postgraduate studies, but the real value of fossils, in numerous ways, must be incorporated into a student's thinking at a very early stage. This also gives rise to some uncertainty about the author's use of stratigraphical time periods, certainly within the more advanced 'boxes', when the stratigraphical table only appears as a small diagram within the text, rather than an appendix, and the periods are not defined in the glossary. It is also unfortunate that the opportunity was not taken to give SI units in addition to the American metric unit conversion table.

To carp, however, at such a comprehensive text, so ably presented, should not detract from an unreserved recommendation of this book for either the first-year non-scientific undergraduate or layman. It is extremely good value, even if the dollar recovers!

D. H. Tarling

D. H. Tarling is Senior Lecturer in the Department of Geophysics and Planetary Physics at the University of Newcastle upon Tyne, UK.

Study of sedimentary rocks

Principles of Sedimentology. By G. M. Friedman and J. E. Sanders. Pp. 792. (Wiley: Chichester, UK, and New York, 1978.) £15.75.

THERE is no doubt that this book is a major benchmark in the study of sedimentary rocks similar to the publication of Pettijohn's *Sedimentary Rocks* in 1949.

Principles of Sedimentology is divided into six parts of fourteen chapters, five complements, a glossary, a bibliography and a subject index. Part I reviews the scope, evolution, techniques and philosophy of sedimentology. Part II describes sedimentary particles. The text has been divided into two courses, course 1 for innumerate, and course 2 for numerate students. As only six pages are considered too mathematical for many students this seems an unnecessary distinction which will increase the neuroses of innumerate readers. Part III deals with sedimentary processes, including deposition and diagenesis. Part IV is concerned with the classification and nomenclature of sedimentary rocks and Part V describes modern depositional environments and the environmental diagnosis of ancient sedimentary rocks. Part VI relates sedimentology to stratigraphy and includes a useful review of the relationship between sedimentation and plate tectonics and an introduction to wireline logs including the Dipmeter.

The next 90 pages of the book consist of sections titled "Complements" A to E. These are actually a series of chapters describing modern depositional processes and environments, including deltas, tidal flats and turbidites. This seems an odd arrangement, as most of this material logically belongs either in Part III (Sedimentary Processes) or Part V (Modern Sedimentary Environments).

The "Complements" are followed by a 22-page glossary which is totally unnecessary. One of the many good points of this book is that each new term is printed in bold face type followed by a definition in italic face. This arrangement, coupled with the excellent index, renders a glossary superfluous. Many of the terms included seem unnecessary—for example, "upwelling, viscosity, wadi, surface texture, stuffed basin (*sic*), very-shallow-water wave"; and does one really need to know that the bushmen of the Kalahari desert call a valley "omiribi"?

The glossary is followed by a biblio-

graphy which is nearly 200 pages long. In their preface the authors say that, in response to popular demand, they have not followed the custom of quoting authors and dates at appropriate places in the text coupled with full bibliographies at the conclusion of each chapter. Instead, sections of text are punctuated by lists of names and dates, but not paper titles. Thus the reader must refer to the terminal bibliography to discover the titles of recommended books and papers. Each chapter then concludes with a short reading list. Though the bibliography is very comprehensive the selected reading lists are less so. It seems strange that, for example, Allen's *Physical Processes of Sedimentation* and Wilson's *Carbonate Facies in Geologic History* are not suggested for further reading at the appropriate places in the text. The bibliography itself is excellent and includes citations up to 1977. An odd

feature of this is the frequent use of the term "*sic*" at an average rate of once a page throughout the bibliography.

Considered overall, the book is comprehensive, accurate, well written and excellently illustrated. Students' minds will not be overtaxed by monotonous slabs of text. Every page contains an illustration or heading to break the monotony. Wiley are to be congratulated for producing a book of this magnitude for a mere £15.75.

Despite the eccentric arrangement of its material there is no doubt that this book will be invaluable to sedimentologists, students, teachers, and industrial geologists working with sedimentary rocks.

R. C. Selley

R. C. Selley is Reader in Petroleum Geology at Imperial College, University of London, UK.

Elementary meteorology

Elementary Meteorology: A Course. By The Meteorological Office. Second edition. Pp. 208. (Her Majesty's Stationery Office: London, UK, 1978.) £4.95.

It is probably not too unfair to say that this book was intended for the education of entrants to the Meteorological Office and is being offered for sale to the general public as a bonus. Perhaps it helps to show that the Ministry of Defence is not only concerned with things secret.

Explicitly, the book is aimed at "weather observers with a background of physics equivalent to that of Ordinary level of the General Certificate of Education"; they will indeed find here much of use and interest.

Chapters on temperature, pressure, water vapour and clouds start from basic physical principles and go on to consider in some detail methods of observation and classification. Chapters on precipitation, thunderstorms and optical phenomena are more descriptive of the processes at work, but the chapter on synoptic meteorology owes much to the early origin of the book and concentrates on correct identification and classification. Forecasting, radar and satellite observation are mentioned briefly. It is a pity that the two rather nice satellite pictures were not dated and do not have an accompanying synoptic analysis.

For this observer, with ordinary level physics, wanting to familiarise himself with meteorological practice and jargon, it is good value for money.

Where else can you find Regnault's equation, the precise authoritative distinction between fog, mist and haze, the definition of *Ci spi cgen* (no kidding—p81) and much else, for less than £5.

Mind, HMSO does manage to produce an incredibly dull combination of typeface and layout that just shrieks 'educational manual', as you open any page. They do not do justice to Pedgley's nice pictures either.

The book is not suitable for the inquisitive, advanced, student who wants to find out what science there is in meteorology—it does not pretend to be and is not an introductory text. There are too many silly mistakes, like saying the pressure "*in pascals*" is given by weight/area, whereas the relationship is true whatever units it is measured in; and that is pretty fundamental physics.

It is ludicrously parochial for a general reader, referring, as nearly as it can, only to weather over the British Isles. Office publications and research are similarly oversold. There is a delightful bit in the Introduction that lists "research into the microphysics of clouds" as one of two "great advances in meteorological knowledge since 1938"—few meteorologists would agree with such an extravagant appraisal. It saddens me to see that the exciting new concepts that have been developed by modern science are going to find their way so slowly and capriciously into the meteorological services.

J. S. A. Green

J. S. A. Green is Head of the Atmospheric Physics Group at Imperial College, University of London, UK.

Fuel and its supply

Energy Resources. By. J. T. McMullan, R. Morgan and R. B. Murray. (Edward Arnold: London, 1978.) Paperback £3.50.

FOUR years ago, in 1975, John McMullan, Roger Morgan and Robert Murray of the New University of Ulster at Coleraine completed a 500-page book entitled *Energy Resources and Supply*, which was published a year later by Wiley. It was a book for graduates and advanced undergraduates, one of the better examples of a genre then burgeoning in popularity. Shortly thereafter the three authors wrote a non-specialist version of the book, only about one-third the length, entitled *Energy Resources*. The paperback version of this second book has now been brought out by Edward Arnold, in their Resource and Environmental Sciences Series, aimed at late school and early university readers. The new book begins with a chapter on energy as an issue; there follow chapters on energy conversion, 'power from natural sources', fossil fuels, nuclear power, and 'inefficiencies'. There is also an appendix on units and an odd and idiosyncratic bibliography.

Energy Resources exhibits many of the virtues of its forerunner. It is readable, written with a light touch and generally good at explanations which convey the shape of a mathematical argument while eschewing the mathematics. The authors do now and then adopt a slightly sneering tone, doubtless intended as jocular but not quite coming off. Anyone who presumes to correct the misapprehensions of others would be better advised, in this fast-moving field, to manifest a certain humility. A great deal has happened since the authors first put together the thesis of their earlier book, and even since the text of their second was finalised in 1976. Their viewpoint, while still widely shared, particularly by official planners, has been overtaken by evolving developments, to such an extent that many of their unstated assumptions have since been scrutinised, analysed and found wanting: for instance their uncritical reiteration that the breeder reactor makes it "possible to use all the available uranium" as fuel, or their distinction between 'weapons-grade' plutonium and any other kind (*all* plutonium is possible bomb material). Chapter 3 on 'energy conservation', says virtually

nothing about the most important stage of energy conversion: that which takes place at the point of end-use of the energy. The book devotes ten pages to nuclear fusion—which does not yet even reliably exist as a controlled phenomenon, much less an energy source—while giving less than two pages to thermal insulation, undoubtedly the energy technology of highest current priority. Recent work, notably the report *A Low-Energy Strategy for the UK*, by Gerald Leach (Science Reviews Ltd and International Institute for Environment and Development: London, £7.50; for review, see *Nature*, 277, 162; 1979) demonstrates the startling potential for improvement in the

end-use energy-conversion infrastructure, to make far more use of both ambient and fuel energy. In retitling the slimline version of their book the authors would have been more accurate to refer neither to 'energy' nor to 'resources' in general, as they deal only with the narrow range categorised as 'fuel' and its 'supply'. As our understanding of the true dimensions of opportunity in energy policy deepens and broadens, 'fuel supply' seems likely to become less and less interesting.

Walt Patterson

Walt Patterson is Energy Consultant to Friends of the Earth (London) Ltd.

Global chemistry

THE consequences of the release of chemicals into the environment, including their chemical pathways and their effects on flora and fauna (including man), have attracted a great deal of attention in recent years, which shows no sign of abating. Much of the concern has followed in the wake of the development of methods of analysis which allow chemicals to be detected in ever smaller concentrations, and it is becoming clear that we live in a most complex chemical soup. Whether this necessarily places us at more risk than our immediate ancestors, however, is not at all certain, although this is one of the questions to which the two books reviewed here address themselves.

R. A. Horne, in *The Chemistry of our Environment* (Wiley: Chichester, UK; £19.40) has set himself a much more ambitious task than J. Lenihan and W. W. Fletcher, in *The Chemical Environment* (Blackie: Glasgow; hardback £8.90, paperback £4.50)—that is, to give an account of the chemistry of the total environment beginning with nothing less than the Solar System. On the whole he succeeds well, and there is a great deal to learn from a study of his book. The writing is as concise as can be expected in a tome of over 800 pages and the text is illustrated by many excellent figures and tables; each chapter also includes an extensive set of references for those with the stamina to explore the subject further.

What spoils the book for me were Horne's not infrequent incursions into polemic, although in his introduction he does warn that he writes both to instil and to provoke. When he does provoke, however, he often also shows the lack of deep understanding which

characterises the instructional parts of his book. Much of the controversial material appears in chapter 12 which deals with "Man's Perturbation of the Biosphere", which includes, in a section entitled "Internal Pollution", an outburst on the evils of drugs, drink and tobacco. Although no-one would suggest that any of these did not present serious dangers to health, I doubt that the present book is the right forum in which to deal with them.

The final section of the book considers whether or not the Earth will survive, at which point Horne finds himself overcome with a sense of futility. Although not convinced that we are all doomed, Horne nevertheless does consider that life on the planet will become very unpleasant indeed, because of man's propensity increasingly to pollute his environment. The pollution he is most concerned with is this context, however, is not chemical but human. Horne believes that the weight of population will finally produce stresses in the environment which are irresistible. This gloomy prophecy seems a long way from the book's original starting point.

The book which Lenihan and Fletcher have edited, in the series *Environment and Man*, is a much slighter work, and although called *The Chemical Environment*, is a highly selective account of some environmental pollutants preceded by an excellent, short survey of the natural cycles of the elements in the environment by Bowen. Those who read Horne's book would do well to use this chapter as their introduction to the subject.

The remaining chapters of this book vary in style and content, but are all interesting and informative and I enjoyed particularly Kipling's elegant chapter on arsenic.

The book does not give anything resembling a complete account of the hazards which chemicals in the environment present to man, nor does it pretend so to do. However, I am surprised that

the editors felt that they could leave cadmium out of their considerations; perhaps they will include it in a future edition.

Both these books contain much that will instruct and interest the student who wishes to know more about the chemistry

of the world in which he lives, and I recommend both to him.

Tony Waldron

Tony Waldron is Senior Lecturer in Occupational Medicine at the London School of Hygiene and Tropical Medicine, London, UK.

Environmental mixed bag

FOUR books on various aspects of the environment, each useful in its own way, are aimed at very different audiences. Sir Alan Cottrell's *Environmental Economics* (Edward Arnold: London; paperback £1.95) is a succinct, readable, but not very exciting, canter through conventional economics as applied to the environment. It contains all that the non-economist needs to know about these in one handy book, but it is a pity he has not covered the 'Materials Balance' approach of Ayres and Kneese, nor the recent work of Pearce, both of whom tackle the aspects of the environment which are not amenable to cost-benefit concepts.

Measuring and Monitoring the Environment (Blackie: Glasgow; hardback £8, paperback £4), by J. Lenihan and W. W. Fletcher, is the best and most interesting of the series *Environment and Man*, but like the others seems aimed simultaneously at diverse audiences, not all of which are equally well-served. In view of the detailed descriptions of methods for chemical analysis of water, and for analysis of hair, it is a pity that some account of biotic indices, as devised by Woodiwiss, Cairns, Chandler, Cowell and others and recently surveyed by Hellawell were not included as an additional chapter. It is a useful source of references for advanced level students and interested lay-readers and for others seeking an introduction to how the environment is monitored and by whom.

A *Dictionary of the Natural Environment* (Edward Arnold: London; hardback £8.95, paperback £2.95), by E. J. Monkhouse and J. Small, is attractive and easy to use, but is heavily oriented towards geographical aspects of environment. For example, 'species' occurs in connection with clouds, but not with organisms! 'Speciation' is not listed, nor are 'biocides';

and 'biomass' should not really be defined as 'an amount of energy'. These are, however, quibbles, and the book will be useful for ordinary and advanced level students and interested lay-readers. Professionals might find it frustrating.

Richard Fiennes' *The Environment of Man* (Croom Helm: London; £6.95) is more serious, and more ambitious than the others. It ranges over all the man-environment issues which have surfaced in recent years: nutrition and food resources; diseases of affluence; stress; genetics and inheritance; and carci-

nogens. For this it is a convenient point of departure for discussion and investigation. Like all such ventures, however, it has been overtaken by time. For instance, widespread undernourishment is no longer attributable to failure of food production to increase as fast as population; and protein deficiency *per se* is no longer identified as the cause of kwashiorkor and marasmus, nor animal protein as of necessarily better quality than a vegetarian diet. Some controversial claims, such as deterioration of the human race when most infants survive to adulthood, and that man is a product of adversity, as though other species were not, are made as though they have wide acceptance. With these reservations the book is interesting and stimulating.

Pauline K. Marstrand

Pauline K. Marstrand is a Senior Fellow at the Science Policy Research Unit, University of Sussex, Brighton, UK.

Shaping the Earth's surface

The Earth's Changing Surface. By M. J. Bradshaw, A. J. Abbott and A. P. Gelsthorpe. Pp. 336. (Hodder and Stoughton: London, 1978.) £5.95.

THIS is in many ways a remarkable book. The authors have undertaken the daunting task of examining the processes that shape the Earth's surface while at the same time stressing the complexity of landforms in terms of form and evolution. As a consequence the range of coverage is immense, extending from plate tectonics to meteorite impact structures (including a brief discussion of lunar surface morphological development) and from surface geomorphological processes by way of anthropogeomorphology to applied geomorphology. In fact almost every topic of relevance to the study of the Earth's surface is touched upon. Much of this information is conveyed visually, there being no less than 215 plates (many of which are in colour) and a similar number of line drawings, mostly taken from recent publications. The approach is thus refreshing and visually attractive.

However, coverage of such a range of topics in 324 pages of text, much of which is taken up with illustrative material, means that depth of analysis has had to be sacrificed. The text is often disappointing, reference-free and

full of generalities and in places seems merely to be providing continuity between the figures. There are, as must be expected, omissions (for example, no mention of the periglacial hypothesis of tor formation or dilatation jointing) and minor errors; and the use of 'boxed sections' for the examination of 'side issues' or 'background information' (for example, continental drift) may be found distasteful by certain readers. The bibliography is also rather disappointing and lacking in structure, being essentially a list of the more readily available geomorphological textbooks.

In spite of these criticisms this book clearly represents a reasonably priced, well presented and welcome addition to the recently much expanded literature about the Earth's surface. It is to be highly recommended to the interested non-specialist as an informative introduction to the subject. It will also certainly find its way into most school libraries and will provide a definite stimulus to both advanced school students and teachers. Its potential value to the university student and specialist is more difficult to define. There is almost certainly something here for everyone, for it provides pleasant browsing, but in itself it lacks sufficient depth for the average first-year undergraduate and must, therefore, be envisaged as a springboard to more advanced and specialised texts.

David K. C. Jones

David K. C. Jones is Lecturer in Physical Geography at the London School of Economics, London, UK.

● The UK edition of *Soviet Science*, by Zhores A. Medvedev, will be published in April by Oxford University Press. For review, see *Nature*, 276, 146; 1978.

● This year's Spring Books supplement will be published on 26 April.

Physical geology

Holmes' Principles of Physical Geology (Nelson: Sunbury-on-Thames, UK; hardback £14.95, paperback £8.50) is one of the classics of geological education and, now in its third edition, has been used by generations of advanced school students and undergraduates. The new edition is substantially the same as its predecessor, though the revolution in plate tectonics has caused it to have its final chapters reorganised and rewritten. There have also been modifications to the sections on batholiths, granite, granitization, ring dykes and migmatites. Some of the figures and plates have been changed. This does tend to highlight the antiquity of some of the material left over from earlier editions, for the years have not treated some of the illustrations kindly. Overall, however, the volume seems to be quite handsome without going in for some of the more flamboyant touches of its American competitors. The references have been updated for this edition though this is more evident in some chapters than in others.

It is not entirely evident that anyone who owns the second edition needs to go out for the third, for there are some major fields in which important recent developments are not reported. The geomorphologist, for example, will find that descriptive and evolutionary (cyclic) geomorphology predominates, whereas the student interested in the Pleistocene will find many discredited ideas (for example, glacial = pluvial) but far too little on ancient ergs or the deep-sea core record. The desert geomorphologist will find that siefs and barchans are still the basic framework for a study of desert dunes and that salt weathering is not considered.

In sum *Holmes' Principles* is now rather like an old sports coat. It is comfortable and familiar, has been patched up rather obviously, and is not such a good fit as it was, but the basic material is still good for a few more years. At £8.50 for 730 big pages the paperback remains something of a bargain.

Introduction to Geology: Physical and Historical (Prentice-Hall: Englewood Cliffs, New Jersey) by W. L. Stokes, S. Judson and M. D. Picard is also a new edition rather than a totally new book. It very much follows the standard format of basic college texts in physical geology and seems to be neither greatly better nor greatly worse than some of its competitors which I reviewed in *Nature* (266, page 100) this time last year. It has been updated by the addition of the usual material that is put into such new additions: something on plate tectonics, something on space, and something on hazards, environment, energy and resources.

The book covers a great deal of

ground in no great detail and by means of relentless subsectioning. The geomorphologist will find some of the geomorphology irritating: rock weathering rates are largely discussed with reference to grave stones, karst landscapes seem to consist of stalagmites, stalactites and water witches, there is virtually nothing on the Pleistocene sequence determined from ocean cores, and there is very little on modern process geomorphology. This is, however, not of course a textbook for geomorphologists but it is disturbing how poor the geomorphic content is. Overall, however, this book is adequate but a little lacking in character.

Students wishing to gain basic instruction in the geomorphological component of this book would do better to go to B. W. Sparks' *Geomorphology*; R. J. Small's *The Study of Landforms*; A. L. Bloom's *Late Cainozoic Landforms*; R. J. Rice's *Fundamentals of Geomorphology*; or A. F. Pitty's *Introduction to Geomorphology*.

The third volume in physical geology to be considered is the third edition of *Puotnam's Geology* (revised by P. W. Birkeland and E. E. Lawson; Oxford University Press: Oxford; £12.25). This book proceeds in a logical way "from a discussion of the materials that make

up the visible earth to the various processes responsible for wearing down and shaping the landscape, concluding with a panorama of earthquakes, mountain building and plate tectonics—in general, the processes that continue to elevate portions of the Earth's Crust".

Of the three volumes this is the one that I liked best. The structure is logical; the illustrative material (including colour plates) is original, simple, clear and striking; there is some attempt at highlighting major concepts and important scientists; and the geomorphological component of the book shows signs of being written by somebody who knows about geomorphology.

By way of conclusion, all three volumes will to varying degrees be acceptable for good advanced school students in geography and geology and they will satisfy some first-year undergraduates in these subjects. Course texts, such as these, are not perhaps used as widely in British Universities as they are elsewhere, presumably because such books have to cover too much in too short a span.

Andrew Goudie

Andrew Goudie is Lecturer in Geography at the University of Oxford, UK.

Metamorphic petrology

Petrology of the Metamorphic Rocks. By Roger Mason. Pp. 254. (George Allen and Unwin; 1978). Hardback £10.95; paperback £5.95.

THIS book is Volume 3 of the *Textbook of Petrology*, which already includes *Petrology of the Igneous Rocks* (Hatch, Wells and Wells) and *Petrology of the Sedimentary Rocks* (Greensmith). It is a most welcome addition to this set and is aimed at about the level of the second-year undergraduate in a three-year geology course. Dr Mason's objectives are fairly modest and largely concerned with a description of what metamorphic rocks are like. For the most part this approach is very successful, aided considerably by the author's evident desire to be understood. The language used is concise but simple, and definitions are given careful attention. A particularly good feature of much of the descriptive writing is the constant supply of useful tips concerning the actual identification and distinguishing features of minerals under the microscope. From the above it should be clear that the affinities of this book lie more with Harker's *Metamorphism* than with more theoretical works, though it is less comprehensive in terms of rock types covered.

Part I of the book consists of three chapters dealing with definitions, meta-

morphism in general, field relations, structures and fabrics. The bulk of the descriptive work is in the nine chapters of Part II. Here some dozen areas are covered in detail to illustrate various types of metamorphism. Contact metamorphic rocks are illustrated by the Skiddaw, Comrie, and Beinn an Dubhaich aureoles, dynamic metamorphic rocks by, amongst others, the Lochseiten Mylonite, lunar breccias and the Ries crater. Regional metamorphic rocks of Precambrian shields are represented by the Lewisian, and metamorphism in Phanerozoic belts is illustrated by the descriptions of the Sulitjelma and Galway regions and by parts of the Alps (for example, blueschists). Most of the above sections are excellently done and form the most useful part of the book. Much of it is highly readable and the author's personal experience of and interest in the rocks comes through. Two short chapters respectively on ocean floor and upper mantle rocks seem less satisfactory, though credit is due for including these topics at all. This central descriptive section of the book will probably make it very popular, for apart from Harker there is no comparable source of good, meaty descriptive writing on metamorphic rocks (the section in the new *Petrology for Students* by Nockolds *et al.* (Cambridge University Press: Cambridge, 1978; hardback £17.50, paperback £6.50) is similar in level but substantially shorter).

The book also has chapters on metamorphic reactions (including a discussion of facies), isotope geology, the use of the electron-probe, and a final chapter on metamorphic rocks and the evolution of the Earth. The theoretical sections are on the whole somewhat condensed and the reader will probably only acquire a superficial familiarity with the topics discussed. For example, in the section on isotopes there is no mention of the fundamental equation of radioactive decay and its derivatives as used in isochron diagrams. Similarly, in discussing metamorphic reactions thermodynamics is more or less avoided. These are both cases where more detail would have enabled problems and exercises to be inserted, probably with a considerable benefit to the understanding gained by the reader.

In summary, the book is rather

disappointing on the topic of metamorphism itself: it is not particularly stimulating, at least partly because it gives very little coverage to some of the fundamental problems of metamorphism such as where the heat comes from and how the study of metamorphism is applied to the understanding of tectonic processes. On the other hand the accounts of metamorphic rocks are rather well done, and the book is worth it for these parts alone. One sympathises with the desire to keep the text short (and cheap) and it may perhaps be impossible to cover all aspects of the subject satisfactorily in a work of this length. None-the-less this book should provide useful background reading for most courses in metamorphic petrology. **K. G. Cox**

K. G. Cox is Lecturer in Petrology at the University of Oxford, UK.

Three branches of petrology

Petrology for Students. By S. R. Nockolds, R. W. O'B. Knox and G. A. Chinner. Pp. 435. (Cambridge University Press: Cambridge, 1978.) Hard-back £17.50; paperback £6.50.

ALTHOUGH Harker's *Petrology for Students* is discontinued after more than 80 years, this book with the same title and from the same stable is obviously its natural successor. It now has three authors, one for each of the main divisions of petrology. With its emphasis on the description of rock thin sections under the microscope it is very much in the Harkerian mould. This impression is reinforced by the use of many of Harker's original 'microdrawings'. From the amount of detailed information presented, the book seems to be most suitable for the second year of a conventional three-year geology course, but as the three branches of petrology are usually taught separately at this level, its success depends on a consistent approach and level of achievement.

The section on igneous rocks (approximately half the book) provides a thorough and systematic account of the petrography of the main igneous rock groups, with useful supporting information on the major element chemistry. The introductory chapter on textures would be more effective if it was illustrated *in situ* (rather than referring to the appropriate figures in later chapters), and the chapter on classification is disappointingly brief and makes no concession to other schemes or to recent attempts to

standardise nomenclature. A more serious drawback is the persistent emphasis on specific examples, many of which come from obscure localities and represent material the average student is unlikely to meet in the flesh. This whole section has a distinctly old-fashioned flavour; the collection of facts is carefully curated, but the presentation is lifeless.

The treatment of sedimentary and metamorphic rocks is generally more effective in that it manages to combine the systematic petrography with some discussion of the rock-forming processes. The sedimentary section contains a useful introductory chapter on textures, followed by a logical and up-to-date treatment of the main groups of terrigenous and chemical sediments. The metamorphic section is less conventional in that a chapter on textures is followed by discussions of metasomatism and melting, facies classification, regional metamorphism, granulites and eclogites, blueschists, and low-grade metamorphism.

This book is quite good value in terms of the quantity of information provided, but it is not altogether successful as a coherent treatment of the three branches of petrology. It would seem more sensible to invest in separate books on igneous, sedimentary and metamorphic rocks, as these generally provide a more complete picture, combining the descriptive material with a discussion of processes and geological context. Without these petrography can be extremely arid.

W. J. Wadsworth

W. J. Wadsworth is Senior Lecturer in Geology at the University of Manchester UK.

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Organic chemistry

IN this section last year, four fat American textbooks were reviewed. This year there are two slim Americans, together with three British books (one large one): two new editions of established works, and a new book written by last year's reviewer, Stuart Warren.

Both of the American texts attempt to cover the main themes of modern organic chemistry, with some mention of biological chemistry, in about 500 pages. One of them, *The Essence of Organic Chemistry*, by J. M. Cram and D. J. Cram (Addison-Wesley: Reading, Massachusetts, and London) succeeds quite well in this endeavour; but the other, *Organic Chemistry: A Brief Survey*, by R. L. Baumgarten (Ronald: New York; £10.25, \$18.45) is rather disappointing. The first six chapters of Baumgarten cover the fundamentals (bonding, nomenclature, and so on), then the middle portion of the book is devoted to the chemistry of the various classes of organic compounds. Finally, more specialised topics are dealt with in eight chapters: these include stereoisomerism, synthetic polymers, primary and secondary metabolites and an overview of spectroscopy. There are an enormous number of problems, but only selected answers are given; and although there are some interesting half-tone illustrations, only rare attempts are made to represent realistic three dimensional structures of molecules. The chemistry throughout is fairly basic, and given the intended readership—students majoring in biology, home economics, nursing, and so on—this is probably all right; but the big disappointment is the lack of emphasis of the relevance of organic chemistry to the Life Sciences. Surely these students should know something about prostaglandins, insect pheromones, and biologically active peptides, to name but a few omissions.

In contrast, the relationships between organic chemistry and biochemistry are nicely handled in Cram and Cram. The first four chapters introduce bonding and structure, etc., through a discussion of hydrocarbon chemistry, and this sets the scene for subsequent coverage of the various classes of organic compounds. Mechanistic considerations are slotted in where relevant, and the illustrative examples are well chosen: the involvement of substitution reactions in terpene biosynthesis; a discussion of the Claisen ester condensation leads on to the reactions of acetyl-thioenzyme A; and many others.

The final four chapters deal with aromatic chemistry, the formation of carbon-carbon bonds, carbohydrates and nucleic acids, and spectroscopy. One glaring omission from this last section is mass spectrometry—in fact, both books are rather weak on spectroscopic methods. Throughout the book discussion of fundamental chemistry is a prelude to coverage of applications to biochemistry. For example, after basic stereoisomerism we have the dynamic stereochemistry inherent in enzyme structure and catalysis; and a section on epoxides includes mention of the sex pheromone of the gypsy moth (an epoxide), and of 2-3-epoxy-squalene as progenitor of the steroids. At the end of each chapter there are summaries of reactions, new terms and topics, and also many useful problems (others are interspersed throughout the chapters) for which answers are supplied. The illustrations are good and include realistic structures for complex molecules.

In summary, although neither book is suitable for degree courses in chemistry, Cram and Cram can be recommended for service courses in chemistry for students of biochemistry, medicinal chemistry, and so on.

The large British book under review is Vogel's *Textbook of Practical Organic Chemistry* (fourth edition revised by B. S. Furniss, A. J. Hannaford, V. Rogers, P. W. G. Smith, and A. R. Tatchell; Longman: London and New York; £17.50). Twenty-two years have elapsed since the previous edition of this book was published, and many new experimental techniques have appeared in this period. This is reflected in the changed emphasis of the present edition. Thus, chromatography increases from seven to thirty-eight pages; and there is a big new section on the interpretation of spectra: infrared and NMR spectra are particularly well covered, and the coverage of mass spectrometry is fair. At the end of the book there are additional correlation tables for IR, NMR and mass spectra (as well as sections on solution characteristics, hazards, and first aid). The bulk of the book comprises a total of 460 primary experiments with many additional cognate preparations, for which full experimental details are given. This is followed by a chapter on qualitative organic analysis, which includes the discussion of spectroscopic techniques as well as all of the classical methods. There are many new experiments, which include: the use of sulphur ylides (one experiment), allene formation (2), cuprates (1), enzymic resolution (1), phase transfer catalysis (1), hydroboration (2), and so on. However, there are several omissions: no

examples of the Baeyer-Villiger oxidation, or the Simmons-Smith reaction, and no mention of the use of alkane dithiols and other reagents which can bring about a reversal of polarity of functional groups (*umpolung*). Perhaps most disturbing is the dearth of Wittig reactions—only one phosphorus ylid is prepared and used—yet there are 31 experiments involving Grignard reagents—surely this excess does not reflect the respective utilities of the two types of reagent. One other disappointment is the total absence of NMR or IR data from the experimental section. Had the chapter on spectroscopy been introduced earlier in the book, and data supplied for each of the compounds synthesised, this book would have been enormously enriched. As it stands the reaction products are characterised by melting and boiling points which, though useful, hardly reflects the modern reliance on spectroscopic proof of structure. The authors state that the book is a one-volume reference text for undergraduates and postgraduates, and with certain reservations this is true, though at £17.25 I doubt whether many individuals will buy it. Certainly all research laboratories should possess a copy, but the people who will find it most useful are those who must devise experiments for chemistry courses.

The remaining books—*Some Modern Methods of Organic Synthesis* (second edition, by W. Carruthers; Cambridge University Press: Cambridge; hardback £25, paperback £7.95); and *Designing Organic Syntheses: A Programmed Introduction to the Synthron Approach* (by S. Warren; Wiley: Chichester, UK, and New York; paperback £3.95)—cover the methodology of synthetic organic chemistry. They are complementary, as the former deals with synthetic methods, whereas the latter shows the reader how to analyse a synthetic problem, and choose the most expeditious synthetic route.

The first edition of Carruthers appeared in 1971, and this new edition has increased in size by about 130 pages. Much of this expansion is due to new sections on organo-sulphur, -selenium, and -silicon chemistry, *umpolung*, and the selective alkylation of ketones; but there are also about twice as many references and a larger index. The basic format remains the same with chapters one and two on the formation of carbon-carbon single bonds and carbon-carbon double bonds. There are much enlarged sections on α -thiocarbanions, and the Wittig reaction, which now includes useful and timely information about sulphur ylides; and new sections on *umpolung* and cuprates. Chapter three covers the Diels-

Alder reaction and includes a new section on the intramolecular version of this reaction, with extended coverage of catalysis by Lewis acids, and of the ene reaction. It also has a new section on the utility of allyl cations, but this is too short, and rather out of date to be of much use. Of the remaining four chapters, the one on organoboranes and organosilanes is particularly good. There are a few disappointments, such as the poor coverage of phase transfer catalysts and crown ethers, and the fact that none of the references are post-1976, and most are pre-1974. Otherwise, the book provides a very good introduction to the methodology of organic synthesis, and should be essential reading for advanced undergraduate courses, and for all postgraduates and others actively involved in organic synthesis.

Stuart Warren's book is a programmed introduction to the design of organic syntheses. It has all of the advantages of a programmed text in that problems are set, and then solved once the reader has had chance to attempt a solution; and the choice of problems is very good. This book represents the first serious attempt to teach undergraduates how to disconnect a target molecule, to provide fragments (synthons) for which syntheses may be discerned. Stephen Turner's book, *The Design of Organic Syntheses* (Elsevier, 1976) is pitched at a much higher level. One particularly appealing feature of the book is the way in which the analysis of many simple target molecules is tackled before the

same methods are applied to complex targets. It is often forgotten that undergraduates are primarily taught synthetic methods, and it is strange at first to think in a retro-synthetic sense. Initially, Stuart Warren takes the reader through the various types of disconnections: one-group, two-group, and illogical (that is, ones where a functional group interchange must be applied before the disconnection can be made). He then passes on to more complex disconnections, such as those involving 'retro-Diels-Alder' reactions, and those involving heteroatoms or small rings; and finally discusses strategy at some length. In fact this section could almost stand by itself as a sort of guide to 'gamesmanship' in organic synthesis. Throughout the book there are review problems, many difficult, which will be of particular utility for lecturers as a source of set work; and most problems have a key literature reference supplied. Any person working through this book will gain a fundamental and very useful understanding of 'synthetic analysis', and it should be essential reading for anyone involved in organic synthesis. My only small reservation is that it might be too difficult for undergraduates, except those taking advanced courses, although their understanding of organic chemistry will be much improved if they can work through it. And at £3.95 it has to be the bargain of the year

John Mann

John Mann is Lecturer in the Department of Chemistry at the University of Reading, UK.

Communicating chemistry

THE large void which separates scientists from non-scientists is initially due to the language barrier. Each particular branch of science has its own linguistic pattern, and communication between practitioners in each branch is often completely incomprehensible to non-scientists. Thus, it behoves the scientist who wishes to communicate with the non-scientist to teach the basic language thoroughly. It is well to recall that we all begin as non-scientists and it is undoubtedly true that many great minds have been lost to scientific endeavour because initial interest was destroyed by a failure of the teacher to lay down a basic framework for the subject. Conversely there are numerous working scientists whose enthusiasm for their subject was nurtured by excellent teachers who could not only teach well but also transmit their own love for, and excitement derived from, their

particular area. (Also, one must admit that many of us can recall teachers who clearly did not understand the principles they were employed to transmit to us.)

Because of the nature and requirements of US degree courses there are several hundred thousand students taking general chemistry courses every year, and probably the majority of these do not intend to eventually major in a science subject. The market for general chemistry textbooks is very large and the competition amongst authors is very keen. This has resulted in some excellent texts being published in the past five or so years. The requirements are often severe, for example, teaching thermodynamics to large mixed-ability classes, but many authors have managed to simplify quite esoteric chemical and physical concepts without loss of integrity, usually with the aid of excellent diagrams and artwork. Thus, a good general chemistry book can often represent the firm basis of an undergraduate's chemical knowledge. At the other end of the

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scale one finds, however, texts which contain little chemistry but much repetition. The best texts I have come across are those written by active researchers, for example, by L. Pauling, H. B. Gray, D. H. Busch; the worst are almost invariably written by 'chemical educationalists'.

The texts reviewed here represent a fair cross-section of the available books, although none are as good as the very best available. The second edition of Fine's *Chemistry* (Williams and Wilkins: Baltimore; \$16.50) admirably exemplifies the wide scope frequently offered.

Fine's book is the most attractive of the present collection, but having said that I shall use it to illustrate some of the inconsistencies which appear in it and some others reviewed here. For example, in a book which includes two chapters on quantum principles and discusses the Schrödinger wave equation why is it necessary to include a section in chapter 1 entitled "Using Powers of Ten" and includes the mind-stretching equation $1.5/10^4 = 1.5 \times 10^{-4}$. Furthermore, a feature of many of these books is contained here, inasmuch as in a book of almost 800 pages only one chapter (37 pages) is given over to organic chemistry. Also, many of the vignettes in the margins of the pages seem to have little to do with text generally and I think are unnecessary. Unlike most of these books Fine's book contains some bad art-work, e.g. the structures drawn on pages 738 and 743.

I enjoyed, too, perusing *Chemistry: A Modern Introduction* (W. B. Saunders, Philadelphia and London; £12) by Brescia *et al.* and similarly admire the wide scope, but am again dismayed to find that nuclear chemistry and organic chemistry warrant approximately equal treatment in terms of pages devoted to them. I was surprised to find in the first two paragraphs of the preface the statements: "This book is written for highly motivated students . . . the work is therefore, in part, intentionally repetitious and written in a language to stimulate students and help maintain their enthusiasm." Repetition will never appeal to the highly motivated student. Again, I suspect that the highly motivated student will wonder why Figure 1.3, which contains an illustration of a rule marked in inches alongside one marked in centimetres, is necessary.

I would find it difficult to recommend E. I. Peters' *Introduction to Chemical Principles* (W. B. Saunders: Philadelphia and London; £10.50). More than the previous two books I find it difficult to know at whom the book is aimed: at those students who can appreciate the quantum mechanical model of the atom (p70) or those

who need to be taught how to count to significant figures or to be told that meter sticks are calibrated in millimetres (p45).

W. L. Masterson and E. J. Slowinski's *Chemical Principles in Qualitative Analysis* (Holt-Saunders: Philadelphia and London; £10.50) is a "student-oriented book", but I can quite truthfully say that I have not in the past ten years come across a book which would in my opinion deaden any interest in chemistry a student had before being unfortunate enough to have to use this book. The fault essentially lies not in the chemistry, but in the attempts to introduce old-style wet methods of qualitative analyses wherever possible, quite apart from the large section of the book (153 pages) devoted to qualitative analysis of cations and anions. Despite the revolution which has occurred in analytical procedures during the past 15 years I am prepared to admit that wet-method qualitative analysis deserves some, essentially historic, treatment in modern books. But I find it depressing that this book makes no mention of NMR or atomic absorption spectroscopy (and little mention of mass spectroscopy), and even more depressing to think that there are Colleges in the US which must still be introducing practical

chemistry to Freshmen by way of qualitative analysis.

G. C. Hill and J. S. Holman's *Chemistry in Context* (Nelson: Sunbury-on-Thames, UK; £5.95) is aimed at students studying chemistry to advanced level lab school, and one can clearly see the influence of American style in the presentation. One might roughly equate the attainment of College Freshmen students in the US with advanced level students in the UK, and thus it is interesting to note that this book devotes 142 out of 518 pages to organic chemistry. I liked this book, but did occasionally find that the periferies are allowed to intrude on, and confuse occasionally, the serious matter of communicating chemistry. Thus, for example, page 1 includes a picture and a biographical note on John Dalton, a reproduction of Dalton's symbols for the elements, an electron micrograph of copper phthalocyanine, and a short quiz on atoms. Incidentally, the thoughtful student will find their d-orbitals splitting diagram on p272 confusing.

C. A. McAuliffe

C. A. McAuliffe is Senior Lecturer in Chemistry at the University of Manchester Institute of Science and Technology, Manchester, UK, and Adjunct Professor of Chemistry at Auburn University, Alabama.

Physical chemistry conceived

CHEMISTRY is a three-dimensional subject. Those incapable of visualising the shapes of molecules represented by two-dimensional structural formulae have to struggle with the topic. The first introduction to this aspect normally comes with tetrahedral carbon bonds and the topic of stereochemistry remains central, especially in organic and biochemistry. The fact that Kurt Mislow's *Introduction to Stereochemistry* (Benjamin/Cummins: Massachusetts; \$11.50) is now reprinted for the sixth time speaks for itself. It was one of the first undergraduate monographs used to supplement a major text and as such outstandingly successful. Emphasis is placed on structural stereochemistry and reactions are avoided. It has three sections; structure and symmetry; stereoisomerism and the separation of stereoisomers. Although explained, it rightly describes hybridisation as "no more than a convenient mathematical fiction".

In *The Chemical Bond* by J. N.

Murrell, S. F. A. Kettle and J. M. Tedder (Wiley: London and New York; £11, \$23.50) hybridisation again takes a back seat, despite its historical importance in the understanding of three-dimensional shapes. This new book, by the authors of the established *Valence Theory* (Wiley: London and New York) is written from the contemporary standpoint with the evidence of the thousands of published *ab initio* molecular orbital calculations in mind. Valence bond and molecular orbital theories are not presented as equal rivals. The fact is that computational ease has left the molecular orbital approach dominant over the alternative, just as the internal combustion engine eclipsed the steam car, despite theoretical objections. Computational techniques are not included and indeed the book demands minimal mathematical skills. Nonetheless any undergraduate, having read it would be much better able to make the transition to the research literature than one brought up on the traditional diet. The topics are introduced in what is perhaps an unfamiliar order (perturbations in chapter 11 and valence bond theory in chapter 13), but this is amply justified. Strong points are the inclusion in an undergraduate text of good accounts of band theory in solids, reactivities of molecules and of intermolecular forces.

The chemist's chief device for coping with the three-dimensional nature of his subject is to use molecular models, although these have limitations as Mislow points out. The models of the title in *Chemistry through Models* by C. J. Suckling, K. E. Suckling and C. S. Suckling (Cambridge University: Cambridge; £15), are interpreted in a much wider manner than mere mechanical analogies. Theories, hypotheses and laws all fall within the definition of a model, so that, for example, the ideal gas law, $PV=RT$, is in this sense a model. The book is subtitled *Concepts and Applications of Modelling in Chemical Science, Technology and Industry* and it thus attempts the rare feat of bridging academic chemistry and biochemistry and industrial application. It is very original, well written and thought provoking. Early chapters introduce models in the general sense and explain the modelling process. The main body of the text then applies models to the concepts of chemistry; quantum mechanics; molecular orbital and valence bond theories; spectroscopy and structure determination; synthesis and reactivity. A biochemical chapter emphasises enzymes but also considers simulations of the origins of life on Earth. Two final chapters treat the design of a chemical plant and innovation, including some account of economic models. The book is highly recommended both as a fresh look at some familiar topics and as an extension of chemistry beyond a student's normal confines. Modelling is a powerful tool which is prone to misuse especially, physical scientists often feel, in the realms of economics or sociology. Here the authors give clear warnings against unimaginative use and suggest how pitfalls can be avoided, all in a very readable fashion.

Physical Chemistry, by I. N. Levine (McGraw-Hill: New York and London; £21) is a late runner in the valuable stakes to find a successor to Moore's *Physical Chemistry* (Longman: London; Prentice-Hall: Englewood Cliffs, New Jersey), thought by some of the pundits to be ageing and capable of being overtaken. Earlier declared challengers were reviewed in this place twelve months ago (*Nature* 272, 101; 1978.). This new starter for the standard undergraduate course in physical chemistry comes from a successful stable and is in every way a thoroughbred. It is hard to find fault but looking at the field as a whole it is impossible not to choose the rival *Physical Chemistry* by P. W. Atkins (Oxford University Press: Oxford; Freeman: San Francisco) as favourite.

Graham Richards

Biophysical chemistry

THE two books under review arrived in the middle of the usual self-examination that follows marking terminal examinations and it was with some relief that I found that the problem of catching and holding the interest in physicochemical problems of biologically orientated students must be unsolved as yet. The authors of *Biophysical Chemistry: Principles, Techniques and Applications* (A. B. Marshall; Wiley: New York and Chichester, UK; £13.25) and *Physical Chemistry: Principles and Applications in Biological Sciences* (I. Tinoco, K. Sauer and J. C. Wang; Prentice-Hall: Englewood Cliffs, New Jersey; hardback \$19.95, paperback \$6.78) appreciate that in recent times several textbooks have been produced which offer recipes for solving this problem, but they must feel (as I do) that these texts all have deficiencies. My overall view of these two new textbooks is that jointly they seem to have good mixes which go a long way to solving the problem.

The authors of both books obviously appreciate that the first requirement is to teach through examples taken from biochemistry and biology using wherever possible familiar problems which lead easily into the physicochemical concepts of molecular action. The descriptions inevitably lead to mathematical formalism, so both textbooks give appendices which summarise the essential components of the mathematics. In addition to the numerous solved examples in the texts, both books give extensive lists of testing problems at the end of each chapter, and, unlike some earlier textbooks, both give answers to the problems, although this means purchasing an additional volume to Marshall's book.

Despite their apparent overlap in the intended audience, Marshall's book is a more specialised text for students of biochemistry studying the wide range of subjects which are collectively called biophysics, whereas the second covers all aspects of physical chemistry for first-year students and omits a lot of the physics given by Marshall. In view of the differences in content, this review may serve a more useful purpose if their details are discussed separately.

Biophysical Chemistry is well written and uses many clearly explained examples from biology to illustrate the principles. Generally the approach has been to take a simple concept, develop this and use the models as a basis to discuss a range of topics throughout

the section. A good example here is the description of the motion of a weight on a spring as an illustration of the concepts of harmonic motion. These concepts are then used to discuss the underlying principles of absorption and dispersion of light, scattering phenomena and transient phenomena such as jump kinetics. This section is one of a total of six sections which cover the laws of thermodynamics, hydrodynamics, kinetics, quantum and statistical mechanics, and one must admire the extensive coverage given to each division.

It must be inevitable in a book of this wide range that the same symbols would be used in apparently unrelated proofs, but occasionally this does lead to confusion. For example, f is used to describe hydrodynamic friction as well as a damping constant for harmonic motion: although the two could have been related through polarisability of the molecules (which incidentally only receives a passing mention), this would not be apparent to undergraduates. One further minor criticism was the omission of transport numbers in the treatment of gel-electrophoresis. Surely this concept would have simplified the understanding of disc(ontinuous)electrophoresis and isotachophoresis.

My own personal sorrow was to see the passing of names associated with equations and concepts: the relationship between electromagnetic intensity and square of the amplitude is no longer called the Poynting theorem, nor are the names Lorenz-Lorenz associated with the relationship between polarisability and refractive index. It always seems more human to have these associations, although I realise that the modern trend is to reduce these connections.

Physical Chemistry is structured differently from *Biophysical Chemistry* and covers both the classical concepts of physical chemistry as well as giving a good coverage of quantum and statistical mechanics. As expected from authors of their reputation, the treatment of these two forms of mechanics is particularly good and could be recommended for all first-year science students. At the end of each chapter is a useful summary of the important relationships derived in the chapter. This makes it easy for students to cross-reference between chapters during revision. I was sorry to see that the Gibbs adsorption isotherm was not included, as it is an old friend which can form a sound basis for discussing hydrophobic effects: possibly this is a logical omission, as hydrophobicity was not discussed, nor were the properties of aqueous solutions given special mention.

Graham Richards is Lecturer in the Physical Chemistry Laboratory, University of Oxford, UK.

Both books seem to avoid the thorny problem of concentration dependence of parameters. Experience shows that one stumbling block for most astute non-physical undergraduates studying physical chemistry is the apparent inability of its theories to deal with properties of solutions at finite concentrations; simply assigning the deviations to virial coefficients (which are not mentioned in *Physical Chemistry*) is not sufficient. I had hoped that the authors of these books may have found a satis-

enthusiastic. fying simple solution for explaining these important solute-solvent interactions. Despite this small disappointment, I feel I could strongly recommend my students to use these textbooks in the future, but I suspect that the cost of Marshall's will be the greatest barrier to all but the most

S. P. Spragg

S. P. Spragg is Lecturer in Physical Chemistry at the University of Birmingham, UK.

Primary and secondary metabolism

PRIMARY metabolites or metabolites that are essential to organisms by providing materials and energy for their maintenance and growth have traditionally been studied by biochemists. *Primary Metabolism: A Mechanistic Approach*, by J. Staunton (Clarendon, Oxford University Press: Oxford; £6.50), takes a chemist's view of the reactions involved in primary metabolic pathways and, as the title suggests, looks at the steps from a mechanistic point of view.

The text commences with an excellent treatment of the main reactions involved in the pathways. The energetics and mechanisms are explained with particular clarity. The role of ATP and other energy-rich phosphates is considered in detail and this is followed by a study of thioesters including coenzyme A derivatives. Subsequent chapters deal with other important cofactors; thiamine pyrophosphate, lipoic acid, nicotinamides and the flavins are introduced and their catalytic roles explained. Current theories on the organisation of the electron transport system are presented in a clear and convincing manner. Here and throughout the book areas under active investigation are indicated.

Having set the scene and described the principal characters, the author describes some of the major metabolic pathways. The catabolism of glucose and its role as an energy source is first described, then photosynthesis and the anabolism of glucose is considered with emphasis on the energetics. The remaining chapters discuss the use of fatty acids as an alternative energy store, transformations of tetrahydro-folic acid derivatives and amino acid metabolism. Mechanisms are discussed for each reaction and the overall strategy and interlocking nature of the pathways are presented in an admirable way that leaves the reader marvelling at the beauty and intricacy of

the transformations.

The book can be confidently recommended to all students of chemistry and biochemistry as a well balanced and refreshing account of primary metabolism. Research workers will also find stimulating ideas for both biological research and organic synthesis within the book.

Secondary metabolites are those metabolites, often called natural products, that seem to be non-essential to the organism. *Secondary Metabolism*, by J. Mann (Clarendon/Oxford University Press: Oxford; £9.75) surveys our current knowledge of the biosynthesis of these metabolites and emphasises the ecological, toxicological and pharmacological properties of the compounds.

The text begins by introducing some aspects of primary metabolism essential to the understanding of secondary metabolism. The second chapter deals with compounds derived from acetate, including prostaglandins and polyketides. Chemical analogies are given for the more important biological reactions emphasising that any theory of biological transformations must be

based on sound mechanistic reasoning. The third chapter dealing with isoprenoids pays particular attention to the stereochemistry of the transformations. Well established results are used for most of the examples and when a pathway or mechanism is not substantiated this is clearly stated.

Aromatic compounds derived from shikimic acid and the alkaloids are discussed in the next two chapters. Metabolites of particular contemporary interest have been chosen to illustrate the biosynthetic pathways. The treatment of the major classes of secondary metabolites is completed with chapter six which concentrates on metabolites of mixed biosynthetic origin. The final chapter gives an enlightening view of the ecological aspects of secondary metabolites and throughout the book the functions of the compounds are stressed to give purpose to the biosynthetic study.

The text is well organised, covers a large area of natural product chemistry suitable for undergraduates and is a valuable introduction for those beginning research in this field. At the end of each chapter there is a small number of problems selected from the recent literature, and the student is referred to the original literature for solutions. The author stresses that it is not a reference text but there is a comprehensive list of such texts in the list of books and reviews suggested for further reading. Overall this is an excellent book and should be very popular.

R. A. Hill

R. A. Hill is Lecturer in Organic Chemistry at the University of Glasgow, UK.

Physics applied to biology

BECAUSE of the complexity and number of variables involved in biological phenomena it is difficult to get precise quantitative data on which to base theories of structure, function and organisation. Yet as understanding progresses it is becoming more common to interpret biology in physical terms, and move towards the goal that "all science as it goes to perfection becomes mathematical in form". Physical and mathematical formulation are the inevitable direction in which the biological sciences must develop. There is a growing appreciation of the need to increase the quantitative and mathematical content of courses in the life sciences. To meet this educational requirement a number of books have been published over the past decade at the introductory, intermediate and specialised levels of physics and mathematics applied to biology.

The present text is at the first level, and although it is claimed to be written for degree and diploma students, who only have a knowledge of ordinary level school physics and mathematics, its content is just above that of an advanced level school syllabus. It broadly covers the field given in the first MB syllabus to Medical students, and, like the latter course, attempts to introduce basic physics by graphical methods without the use of calculus. There are a number of good textbooks available in this field and any new addition will as a consequence be subject to very critical appraisal. In curricula content it is well balanced, starting with a chapter on force, motion and energy, followed by chapters on waves, atomic and molecular structure, electricity, light, heat, and finishing with radioactivity and radiobiology. Most of the text is precise, and in respect of descriptive material well presented. It is with the problem of introducing new concepts and conveying an understanding of the principles of physics that the book

is weak. Particular areas that need revision are the meagre introduction to capacitance, inductance and AC theory; the description of phase contrast microscopy with terms such as "this is done by inserting a glass phase ring in plane P", with no explanation of phase rings, will leave students who have only the basics floundering; and in a similar manner the introduction to electron microscopy and the wave nature of moving electrons is badly skimmed. Throughout the text there are curious statements which suggest that the book was written to meet a deadline. On page 5 it is stated, "It is a common experience that when a stationary object is pushed sufficiently hard it moves".

Later (p 35), we have, "The eye is sensitive to light because the retina responds to the electric field". About ionic conduction through membranes it is stated (p 84), "The permeability of the membrane for Na^+ ions is less than that for K^+ and Cl^- ions so that when the fibre is in its normal state there are more Na^+ ions on the outside than inside". There are too many statements of this kind to unreservedly recommend it for reading by biologists as an introduction to physics.

John A. Sirs

John A. Sirs is Reader and Head of the Department of Biophysics at St Mary's Hospital Medical School, University of London, UK.

Insights into biological phenomena

Introductory Biophysics. By F. R. Hallett, P. A. Speight and R. H. Stinson. Pp 243. (Chapman and Hall: London 1978.) Paperback £5.95.

THERE is currently a need for a comprehensive modern textbook on biophysics suitable for university students. There are, of course, many books which are in effect physics primers for biologists, but

what is required is rather a book that illustrates the great insight into biological phenomena that can be gained from the application of physics. This book falls into the latter category. It not only describes the application of classical physics to biological phenomena but also covers the more modern areas such as the biological effects of electromagnetic and ionising radiations and the application of X-ray crystallography to biological molecules. Thus, physics is brought in as a tool to solve problems rather than as an end in itself. One can criticise the book for omission of some important topics—forexample, thermodynamics and respiratory processes—but for a book of its size it covers a reasonable range. Furthermore, it is good to find a book aimed at the life science student which does not attempt verbal gymnastics in order to avoid a mathematical description of a process, and one that lays considerable emphasis on problem solving to consolidate concepts in the mind of the student.

Unfortunately, although the book has many good points, it is nevertheless something of a disappointment on two counts. The first is a lack of balance apparent in some of the chapters. It is, for example, difficult to understand how a chapter labelled 'molecular biophysics' can contain a discussion of protein structure and yet not show even one diagram of a protein model, and contain only one paragraph on how the modern understanding of the architecture of such molecules allows an interpretation of their function at a molecular level. Similarly, in the chapter on radiation biophysics, a good section on target theory is followed by a trivial discussion of the actual damage that ionising radiations can cause; furthermore, in such a chapter surely the biological and medical applications of ionising radiations deserve mention.

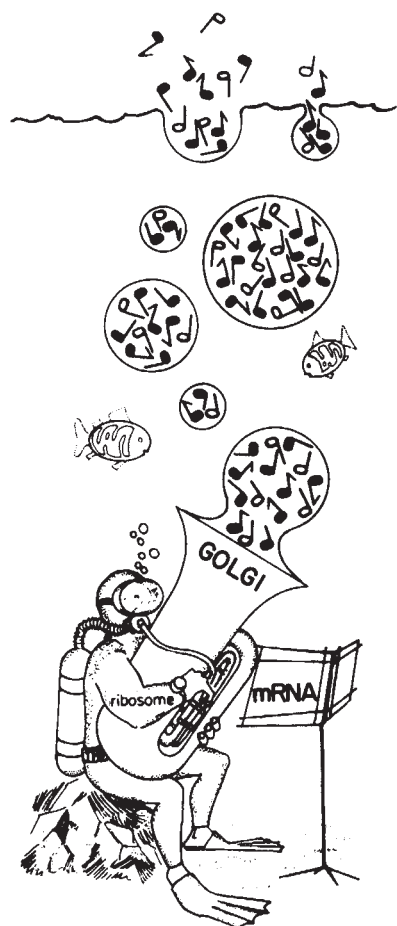
A second area of disappointment lies in the terse manner in which many crucial concepts are covered. This may be acceptable in a revision book, but this is clearly meant to be a primary text

and as such these concepts deserved a fuller treatment. To give only two examples: if, as occurs in chapter 7, it is necessary to have a section explaining the physical meaning of the terms work and energy, then more than the two paragraphs given are needed for adequate coverage. Similarly, in the chapter on heat flow in biological systems the phenomena of convection and radiation are explained at some length but conduction is not explained at all except in so far as the relevant equation is given. Indeed this particular chapter is rather poor because, though many of the ways in which heat may be lost by an animal are explained, the control of heat production and loss in animals is described in so disjointed and incomplete a manner that it would be difficult for the student to obtain a coherent picture.

So we are still in need of a really good textbook on Biophysics, for, although this book does contain some very good material, it is frequently marred by omission and inadequate presentation.

M. W. Rampling

M. W. Rampling is Senior Lecturer in Biophysics at St Mary's Hospital Medical School, University of London, UK.



Taken from Membranes and their Cellular Functions, reviewed on page 88

Lysosome

This Publication . . .



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Bioenergetics

THE main effort of research in bioenergetics, as shown by a glance through a journal such as *Biochimica et Biophysica Acta Bioenergetics*, is at present devoted to the biochemical details of energy transfer in mitochondria, chloroplasts and bacteria. However, underlying these processes are the basic physical principles embodied in the laws of thermodynamics (from which the concept of energy is derived), and quantum mechanics. These principles are the subject of *Foundations of Bioenergetics* (Academic: New York and London; \$26.50; £17.20), by Harold J. Morowitz.

The teaching (or rather the learning) of thermodynamics always seems to entail special difficulty. It is as if a deep gulf divides the majority of humankind who are baffled by, or ignorant of the subject, and those who feel they understand it. Morowitz leads the way across this gulf with confidence, carefully explaining where all the postulates of this strange discipline come from. Statements are punctuated by (definition), (empirical generalisation), (postulate) or (conclusion). The introduction of the method follows the conventional way of physical chemistry; steam engines and information theory both feature in the description of entropy. The treatment is quite rigorous. Biologists may be daunted by the variety of mathematical methods that are used, from calculus to Boolean algebra, but these are all given some introduction.

The book then continues into statistical mechanics, approached from a quantum mechanical viewpoint. Frequent references are made to the relevance to biological systems, from cells to ecosystems. The power of thermodynamics and statistical mechanics lies in their ability to draw wide-ranging and sometimes unexpected conclusions on topics such as the origin of life. In a section, "Life at absolute zero", he notes that "structure is the result of where the molecules [in a living system] are located, and function is a description of where they are going". From this, and the observation that simple life forms can survive freezing to very low temperatures, we are forced to the conclusion that the process of originating living material reduces to a problem of synthetic chemistry. Finally, it must be admitted, however, that there are severe limitations to the applicability of equilibrium thermodynamics (or 'thermostatics') to biological systems which are in a state of flux, and the later chapters cover fluctuating systems and the approaches to irreversible thermodynamics. These too have their limitations, caused by the complexity and non-ideal behaviour

of living systems, but the book provides a good introduction to these potentially useful methods.

In the 15 years since the publication of Lehninger's classic monograph *The Mitochondrion* (W. A. Benjamin: New York and Amsterdam), there have been considerable advances in our understanding of this organelle. These are documented in many books for the specialist and symposium proceedings, but it is surprisingly difficult for undergraduate students to obtain a straightforward overview of the present state of the subject. *Mitochondria: Structure, Function and Assembly* (Longman: London and New York; £3.75), by Peter A. Whittaker and Susan M. Danks, is designed to fill this gap. In price and length it is the sort of book that students of biology and biochemistry are likely to buy and read through. In condensing a large amount of research data, however, inevitably some of the excitement is lost. In reading the section on the mechanism of coupling between respiration and phosphorylation, there is only a faint echo of the spirited debate that has enlivened the Biochemical Congresses of the past ten years. The three major hypotheses are described and their merits are discussed; and it is con-

cluded that Mitchell's chemiosmotic theory is the most acceptable explanation of the available data (a conclusion recently endorsed by the Nobel committee). Oxidative phosphorylation may be the keystone of mitochondrial function; but despite the attention devoted to it recently we should not forget that many metabolic processes take place within the organelle, and here these are accorded their due weight.

The book is divided into five chapters: structure and isolation of mitochondria; tricarboxylic acid cycle and fatty acid oxidation; oxidative phosphorylation; transport across membranes; and mitochondrial assembly. The descriptions are punctuated by brief details of experimental methods, and general references for further reading are given at the end of each chapter. Advanced students may find that they need a more comprehensive set of references and will need to plunge into the specialist literature. However, this is a well balanced account, up-to-date and without frills. It should be a bestseller.

R. Cammack

Richard Cammack is Lecturer in the Department of Plant Sciences at King's College, University of London, UK.

Membrane functions

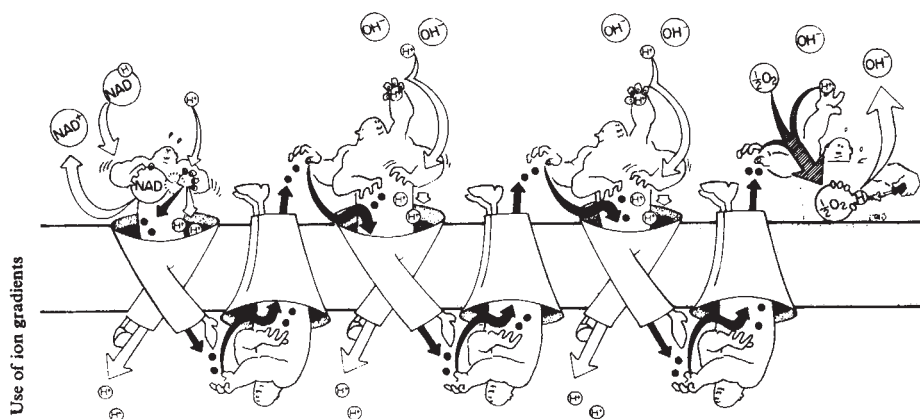
Membranes and their Cellular Functions. Second edition. By J. B. Finean, R. Coleman and R. H. Michell. Pp. 157. (Blackwell Scientific: Oxford and London, 1978.) Paperback £4.80.

OPENING the first edition of *Membranes and their Cellular Functions* was a great surprise as one found the excellent cartoons by T. A. Bramley. The authors are doing a valuable service in showing the reader that scientists are not deadly serious data-processing robots. We should also remember that students need some light relief during the work of absorbing the great mass of information presented to them. The sympathetic attitude of the authors also comes over in the text; they are not trying to impress with their sophistication but are simply and clearly telling the reader about biological membranes.

The first edition was excellent for supplementary reading but I felt that some of the more molecular aspects received rather scant treatment. In this new edition, although the original material and style has been retained, the subject matter has been re-arranged and not only brought up to date but considerably expanded, particularly in the molecular aspects. For example,

membrane structure and composition, mechanisms of solute movement across membranes—including the ion-translocating ATPases, energy coupling by ion gradients, neuro- and hormonal transmitters, and recognition responses—are now fully described. The result of this revision is the ideal book to cover the membrane section of a first- or second-year course in biochemistry, physiology or cell biology. It fills a gap at this particular level; Harrison and Lunt's *Biological Membranes* (Blackie: Glasgow and London, 1975) and Quinn's *The Molecular Biology of Cell Membranes* (Macmillan: London, 1976) being more detailed and suited to final-year courses, whereas Davies's *Functions of Biological Membranes* (Chapman and Hall: London, 1973) is rather elementary and now somewhat dated.

Our second-year students will be left in no doubt that they should read this book. Any students who have not encountered a course on membranes should read it in the long vacation before a final-year course in biochemistry, physiology or microbiology. Excellence does not mean perfection and there are, of course, some faults. What is called the Nernst equation (p86) is really the Henderson equation for diffusion potentials; the Nernst equation for equilibrium potentials is not given. This omission may explain the misconception on p75 where it is stated that the distribution of Rb^+ ions



Use of ion gradients

Taken from *Membranes and their Cellular Functions*, reviewed on this page

"at equilibrium neutralises the $\Delta\psi$ (membrane potential)"—if it did it could not be used to measure the membrane potential. On p44 the unfortunate Nernst suffers again, the $\frac{d\psi}{dx}$ being described as the charge, instead of the electrical potential, gradient. In my own research field, mitochondrial bioenergetics, I would quibble with the authors' selection and acceptance of some of the very recent data. I am sure others will find the same with their own speciality but this will not seriously affect the student or non-specialist reader: if it stimulates discussion with lecturers so much the better.

A more general criticism is that abbreviations tend to be thrown into the text leaving the reader to search

figures or tables to find their meaning. Also, although the lists of further reading at the end of each chapter are suitably short and carefully chosen, I felt that more help should be given to students in relating articles to particular topics. Further use of the wide margins could be made for both this and explanation of abbreviations.

These are all relatively minor criticisms and do not alter my view that this is now a really excellent book. It is accurately printed in attractive typeface on good paper making it, with the authors' style and the illustrations, a real pleasure to read.

M. J. Selwyn

Michael J. Selwyn is Senior Lecturer in the School of Biological Sciences, University of East Anglia, Norwich, UK.

Biochemistry texts

THREE recent biochemistry texts reflect different approaches to the student market.

The second edition of *An Introduction to Practical Biochemistry* (McGraw-Hill: New York and London, £6) is wide-ranging in its scope, describing in all some 140 simple experiments that can be carried out in any reasonably equipped biochemistry teaching laboratory. It is simply and clearly written. Descriptions of experimental procedures are interspersed with accounts of the appropriate underlying principles. In some cases I feel that this aspect of the book is rather overdone and would best be left to a general textbook. Experiments are adequately described together with lists of quantities of reagents and equipment needed for a given number of students. The book leans heavily to the physical and chemical rather than the physiological side of the subject. There are sections dealing with pH and buffers, separation methods (dialysis, gel filtration, column chromatography, partition chromatography, electrophoresis), colorimetry and spectrophotometry, protein isolation and N-

and C-terminal determination, artificial membranes, nucleic acids (base-composition, isolation and estimation), simple enzyme kinetics and also rather extensive sections dealing with qualitative and quantitative tests for sugars, amino acids and lipids. The final chapter, entitled "Metabolism", contains experiments of the more physiological type including isolation of mitochondria and chloroplasts, adipocyte isolation and metabolism, intestinal transport of amino acids, glucose fermentation by yeast, bacterial growth and β -galactosidase induction and a few whole-animal experiments. In general, quite a good book that is suitable for use in the first two years of a biochemistry degree course.

Basic Biochemistry: A Visual approach for College and University Students (Heinemann Educational: London, £2.90), by J. Edelman and J. M. Chapman, might appeal to some who like a smattering of biochemistry presented in 'comic-strip' form. Personally, I hated it. The book is in three sections. The first deals with the components of the cell, proteins, carbohydrates, lipids and nucleic acids, and in my view is the best part of the book. From this an elementary student should gain some feel for the basic structures in the cell, without gaining

such idea about their chemical properties and reactions or the chemical principles underlying these. The second section deals with enzymes and attempts to explain enzyme kinetics and action with the aid of a multitude of diagrams (which in many cases are rather scrappy) and no mathematics at all. I think a student is more likely to be confused rather than informed by this section. The third and largest section deals with metabolism. Here again, the use of a plethora of rather scrappy diagrams seems more likely to sow confusion than understanding.

The treatment of much of intermediary metabolism is scimpy and there is no attempt to set it in a physiological framework. There is also a heavy use of terms such as 'high-energy bond' 'energy release' and 'energy trapping' which often are not appropriate. This book might be suitable for a student with little previous chemistry who is required for some reason to acquire a veneer of biochemistry, otherwise, it is not suitable as a serious biochemistry book.

Amino Acids, Peptides and Proteins: An Introduction (Macmillan: London; £15), by H-D. Jakubke and H. Jeschkeit is really a specialist book which might be of use as a reference text for final-year undergraduates taking an honours degree in biochemistry or chemistry. Visually, the book is rather unattractive, having plain diagrams and a small type-face. On the other hand, the amount of information it contains is considerable. The book falls into three parts. The first deals with the amino acids; their classification, physical properties, synthesis, analysis and reactions. The second part covers peptides—in particular, synthesis (problems encountered in the latter, general strategy, together with detailed methods). In fact one third of the entire book is given over to peptide synthesis. There is also an interesting section dealing with an extensive range of important naturally occurring peptides. The third part deals with proteins and covers classification, physicochemical properties, isolation and purification, sequence analysis and structure. The text is quite readable and does not seem to have suffered in the course of translation from the German version. The book is interspersed with frequent reference sections which by now are not very up-to-date (1972–74 are the latest references). In conclusion, a useful book, more to be used for reference than as a student's reading book.

E. D. Saggerson

E. D. Saggerson is Lecturer in Biochemistry at University College, London, UK.

Insect biochemistry

Biochemistry of Insects. Edited by M. Rockstein. Pp. 649. (Academic: New York, San Francisco and London, 1978.) \$29.50; £19.15.

IN 1961 Darcy Gilmour's book *The Biochemistry of Insects* (Academic: London) was published and in a text of under 300 pages it covered the available information on insect biochemistry, and included a generous amount of background information on general biochemical principles. The growth of the subject in the intervening years has been prodigious and to effectively review the field in a book that is little more than twice the size of Gilmour's is no mean achievement. The book that Morris Rockstein has edited covers the ground admirably. Its production and format closely resembles the six-volume revision of *The Physiology of the Insecta* produced by the same editor and publisher in 1974, and these two works will stand as a major source for entomology students for many years to come.

The majority of undergraduates who undertake specialist entomology courses these days will have received some basic biochemical training and this is essential if maximum benefit is to be derived from this book. However, it is not simply an exposition of the biochemical specialisations to be found in the *Insecta*. The authors have each taken the trouble to fit the biochemical facts into the wider biological picture and no matter what aspect of entomology interests the reader, he or she will find valuable facts and ideas within this book.

Inevitably, in any work which is compiled from several contributing authors there is some lack of uniformity in approach. To quote one example, the chapters on carbohydrates and lipids deal with the nutritional requirements for these compounds, and with their digestion and absorption, but no corresponding information is available in the chapters on proteins and amino acids. A lack of uniformity is also evident in the references to original literature, the chapter on insect biochromes having a bibliography of more than 30 general references and about 140 specialist references, whereas that on the toxic action of insecticides has only five general references. The separate listing of general works in the bibliographies is an admirable idea for the undergraduate reader interested solely in reviewing a particular topic. On the other hand the postgraduate reader who consults those chapters where

specialist references are lacking, will require access to several other reference works before the original papers can be tracked down.

This very readable book is a valuable and significant contribution to the entomological literature. It provides important background information for

undergraduate courses and will be a standard reference work for a wide range of professional entomologists.

John B. Ford

John B. Ford is Senior Lecturer in Entomology in the Department of Applied Zoology at the University College of North Wales, Bangor, UK.

Biochemistry of viruses

The Biochemistry of Viruses. By S. J. Martin. Pp. 145. (Cambridge University Press: Cambridge, 1978.) Hardback £10.50; paperback £3.95.

KNOWLEDGE of the nature and biosynthesis of viruses has progressed to a stage where it is clearly impossible to encompass all aspects of the biochemistry of viruses in the space of 145 pages. The author of this latest volume in the series *Cambridge Texts in Chemistry and Biochemistry* has adopted a sensible compromise presenting a coherent survey of the subject based largely on his own experience and interests. The text progresses from a brief but adequate review of the history of virology through chapters on classification, assay, purification, structure, strategy of infection (that is, transcription, replication, maturation, defectiveness), therapeutic and prophylactic agents, and evolution. The book begins and ends on a philosophical note contrasting the virus as the invader of the genosphere with man as the invader of the biosphere.

The best and most comprehensive portion of the book is the chapter on virus architecture. The chapter on strategy of infection, which takes up 54 of the 145 pages, is well written but is less satisfactory, as it fails to deal adequately with the animal DNA viruses and the RNA tumour viruses. The text also suffers from having been prepared before the application of restriction endonucleases to analysis of

the structure of the genomes of DNA viruses. In my view these deficiencies, together with the virtual exclusion of genetics, will limit the usefulness of this book as a course textbook. It can be recommended, however, as supplementary reading because Dr Martin has a lucid and simple style of writing which provides the hard-pressed student with an excellent example of the organisation and presentation of information in essay form.

Over-simplification occasionally generates inaccuracies such as "all RNA tumour viruses . . . appear to have similar base sequences" (p123); "most DNA viruses contain circular DNA molecules" (p44); and the like. Also a number of errors have slipped through. For instance, part of Figure 5.26 is missing; two eminent virologists have their names mis-spelt; the molecular weight of herpes simplex virus DNA is given variously as 54.92 (Fig. 2.4), 75 (p92) and 80-100 (Appendix) million and that of oncornavirus RNA as 10 million (Fig. 2.5). The retroviruses do not appear in the virus classification in the Appendix, apart from Rous Sarcoma virus which is listed as the prototype of the genus coronavirus of the family rhabdoviridae!

These imperfections will be sufficiently obvious to most readers not to detract from the basic worth of the book as a simple introduction to the biochemistry of viruses.

C. R. Pringle

C. R. Pringle is a member of the scientific staff of the Medical Research Council Virology Unit in the Institute of Virology, University of Glasgow, UK.

Molecular genetics

THERE have been three exceptional textbooks written about microbial genetics. These are *The Genetics of Bacteria and their Viruses* by W. Hayes first published in 1964, *The Molecular Biology of the Gene* by J. Watson first published in 1968, and *Molecular Genetics* by G. Stent which was first published in 1971. These books cover a lot of similar territory, but each has a distinctive flavour. In Hayes' treatment the development of the subject

seems a trifle disorganised in places, but the book is written with a sense of enthusiasm and wonder which is quite infectious. Watson's approach is autocratic, or "sovereign didactic" to quote the felicitous if somewhat ambiguous description from Stent. One has the feeling that no matter how cells originally functioned that after reading Watson's book they would have considered it advisable to conform to the royal dogma. Stent's book is undoubtedly the most literary of the three. He set out to write an introductory narrative and the book tells the story superlatively well.

Of these books Hayes came out in a

second edition in 1968 so is now in need of revision, Watson had a third edition published in 1976, and a second edition of *Molecular Genetics* (Freeman: San Francisco and Reading; hardback £14, paperback £9.70) has been prepared by Stent in collaboration with R. Calendar. The new edition retains the greater part of the original text (605 pages) and adds to it about 150 pages of new material either in the form of extra paragraphs or plates inserted at strategic places in the narrative, or by completely new sections added to relevant chapters. An examination of the small modifications which have occurred to the old text gives a fascinating insight into some alterations which have happened over the years to scientific language interpretation and technique. For example, the heading "The sex factor as an episome" has become "The fertility factor as a plasmid" and in the same vein "The conjugal tube and mobilisation" is now "The conjugal bridge and mobilisation". Also, the classic autoradiograph of a bacterial chromosome by Cairns has been re-interpreted to allow for bidirectional replication, and some unspecified development has introduced substantial changes to the curve depicting the kinetics of ONPG hydrolysis by β -galactosidase resulting in a threefold drop in affinity constant. One sad loss, no doubt inevitable because of cost considerations, is the four colour plates of the original edition, particularly that by I. M. Wallace of eye-coloured mutants in *Drosophila*, which is a work of art in its own right. A less comprehensible omission is that of the Newcombe resspreading experiment. This is the example I would propose to students if asked to name a single experiment as an ideal they should aim for in their research. It is ingenious, simple in principle and practice, and unambiguously proves its point. When this edition is reprinted perhaps the missing two pages could be added as a supplement by way of expiation.

The more extensive new sections are written in the same style as the original and have up-dated the text on a selection of topics including DNA replication, gene expression, RNA phages, genetic engineering, transcription and translation. However, the limited scale of the revision couldn't hope to do justice to all the advances that have occurred in the past 8 years, so there is little discussion of recent developments in a number of other areas—for instance, in DNA repair, plasmids or transposons. Nevertheless this edition is the most useful text available at present for teaching introductory courses in molecular biology. It is not, however, a contemporary classic like its progenitor was in its time. There is still a real need for a completely new and comprehensive treatment of the subject.

Two books by D. Freifelder are more specialised aids to teaching. *Molecular*

Biology and Biochemistry: Problems and Applications (Freeman: San Francisco and London; paperback £4.10) is a useful text in the manner of *Biochemistry: A Problems Approach* by Wood, Wilson, Benbow and Hood (W. A. Benjamin: New York). Each of 17 chapters starts with a concise introduction to a particular topic, and this is followed by some references and a collection of relevant problems. Answers to the 550 problems are supplied at the back. The book will be of considerable use to both elementary and advanced students.

The DNA Molecule: (Freeman: hard-

back £13.40, paperback £7.40) contains a collection of 44 original articles arranged in sections, each of which has a short preface and is concluded with some pertinent questions and references. The collection has a structural rather than genetic bias and although it will be a useful reference book for microbial geneticists its role in teaching is more likely to be in courses of biophysics or structural molecular biology.

N. Symonds

N. Symonds is Professor of Microbial Genetics at the University of Sussex, Brighton, UK.

Textbooks of genetics

Most textbooks of genetics seem to fall into three main categories: those designed for an educated general readership, those designed for university students taking courses in which genetics is not a major topic (for example, social sciences and medicine) and those designed for students with a specific interest in genetics. However, Sam Singer's slim (139 pages) *Human Genetics: An Introduction to the Principles of Heredity* (Freeman: San Francisco; paperback £3.10) does not fit easily into any of these categories. It is a very nicely presented summary of some of the essentials of human genetics, but it is difficult to know for whom it might be of value. It is too technical for the general reader or advanced school student and yet would be inadequate for an undergraduate course in human genetics. This is a pity because the author is clearly equipped to write a fuller text which undoubtedly would be very appealing to undergraduates. On the credit side I would rank it highly in being remarkably up-to-date, accurate and very readable. The illustrations are simple but of a particularly high quality. This is presumably a reflection of the publishers' association with *Scientific American* which features prominently among the suggested readings at the end of each chapter each of which also concludes with a summary. There is an Appendix of 25 problems (with answers) mainly concerned with what is often loosely referred to as "pedigree genetics".

Two recently published books on population genetics illustrate very well the differing approaches to the subject. One is designed for the relatively non-specialist undergraduate (Bryan Shorrocks; *The Genesis of Diversity*; Hodder and Stoughton: London; hardback £5.75, paperback £2.95), the other for the specialist undergraduate or postgraduate (Eliot B. Spiess; *Genes in Populations*; Wiley: Chichester, UK, and New York; £13.95). I would warmly recommend both of these books to the readership to which

they are directed. Both deal with factors affecting gene frequencies in populations, natural selection, polymorphisms and the underlying genetic basis of individual variations. But whereas Shorrocks concentrates more on the biological aspects of variation and requires no more than a basic knowledge of simple algebra, Spiess' approach is largely based on mathematical reasoning. Regarding the *Genesis of Diversity* most examples are chosen from *Drosophila*, a field in which the author has made his own contributions. Industrial melanism in moths (*Biston betularia*) is also given special attention. It seems odd that there is no mention of Clarke's work on frequency-dependent selection (though the subject itself is discussed), and there is very little about the population genetics of man. The latter is particularly unfortunate because this is an aspect of the subject which is rapidly growing in interest and importance. The presentation is clear and is well illustrated with line drawings and graphs. There is a list of well chosen references and an index. In its field it will have a number of competitors but as a straightforward and well written introduction to the subject it will no doubt be welcomed by many students and their teachers.

Whereas Shorrocks' book can be read from cover to cover in a couple of evenings, Spiess' book would defy any such approach. It is, at least to me, a book to dip into. A knowledge of differential calculus and matrix algebra, though not essential, would certainly make for a fuller appreciation and understanding of the text. There is, however, a very helpful appendix of various mathematical and statistical formulae used in the text. This too is a very attractive book and the way the material is clearly divided into well defined sections is very helpful to the reader. Also, examples are chosen from a wide range of organisms including man. Unfortunately, though each chapter concludes with a number of questions, no answers are provided, which is unhelpful to students and a possible source of embarrassment to teachers.

Alan E. H. Emery

Alan E. H. Emery is Professor of Human Genetics at the University of Edinburgh, UK.

Contemporary microbiology

Two recently published microbiological textbooks are aimed at opposite ends of the student market. One comes close to the bull's-eye of its target. The other discharges its material with all the accuracy of a sawn-off shotgun. *Essays in Microbiology* (Wiley: Chichester, UK; £13.50), edited by J. R. Norris and M. H. Richmond, is a set of sixteen chapters (not, in my opinion, essays) of exactly equal length. The readership is expected to be "the early stage undergraduate student with some topics of relevance to the senior school pupil". *Companion to Microbiology* (Longman: London and New York; £12), edited by A. T. Bull and P. M. Meadow, consists of nineteen chapters of varying length and format covering a wide range of topics chosen for a readership of "advanced undergraduate and postgraduate students".

The editors of *Essays* have declared as one of their objectives a need to inform the undergraduate embarking on a microbiology degree course just what he is letting himself in for and they wisely achieve a balance between the descriptive and molecular aspects of the subject. The editors of *Companion* have solicited articles from experts in some of the growth areas of microbiology and have included topic neglected in recent textbooks "for discussion in broader perspective than is usual in review articles and in more depth than is possible in a general textbook". The latter formula sounds very much like that of an essay and, indeed, the chapters in *Companion* are very much more like essays than are the sections in *Essays*.

With new texts for the student market appearing with a frequency approaching that of new bacterial species in the hey-day of descriptive microbiology, the reviewer must clearly advise the potential consumer as to whether the various objectives of the editors and authors have been achieved and whether the publishers have been able to offer value for money. It is difficult to avoid a comparative approach when reading two books such as these and, at the risk of being unfair to *Essays*, I feel obliged to comment that *Companion* is more successful in achieving its stated objectives and much better value for money than *Essays*. Having made this judgement, and because the articles in *Essays* are on sale separately I shall devote more space to this volume than to the other.

Professor Stanier's contribution, "What is Microbiology?", maintains the high standard we have come to expect from this source and the article by one of the editors, Professor Rich-

mond, on "Genetic organisation in bacteria and its expression" is equally successful. Four chapters on form and function in bacteria, fungi, viruses and protozoa are of variable quality. Dr Murray's article on bacteria adopts a cytological and ultrastructural approach but there is some careless proof-reading which detracts from its intelligibility in places. The chapter on fungi (Dr Booth) is a dull, detailed descriptive survey which will not generate much enthusiasm for mycology amongst younger readers. The viruses are better served. Professor Watson succeeds admirably in presenting the intricacies of virus structure, particularly that of icosahedral viruses, in a lucid manner. Herpes may not be so simplex these days but the author manages to express the most complex geometrical concepts in a manner which would be understandable to an advanced school student and stimulating to a final-year undergraduate. Alas the protozoa suffer much the same fate as the fungi, although the authors (C. R. Curds and C. G. Ogden) are mercifully less obsessed with taxonomy than Dr Booth. The chapter begins rather well with the taxonomy compressed into a page of suitably small print, but one soon encounters phrases like "In tectinous allogromiid foraminifera . . ." without either warning or explanation. Such experiences tend to have a somewhat anaesthetising effect on the non-specialist.

One curious feature of these two books is that one of the co-editors of each has an article not in his/her own book but in the other, which all goes to emphasise what a small universe we microbiologists inhabit. Thus Professor Norris writes for *Companion* and Dr Meadows, a co-editor of *Companion* writes on "Chemistry and composition of micro-organisms" in *Essays*. Her contribution gets to grips with the structure/function relationship very well and a reader with a good background in structural biochemistry could gain much by reading it. However, readers with ordinary level German will be surprised to learn that the nomenclature of O and H antigens derives from "the German *ohne Hauch*, without whip"! I think that the intended readership would find Professor Tempest's approach to "Dynamics of microbial growth" rather dull and theoretical. He considers the ecological implications of substrate-limited growth only very briefly and some mention of the growth rates of organisms in their natural environments or the implications for industrial microbiology would have made the chapter more interesting. "Intermediary metabolism" is covered by Professor Clarke in a clear and interesting account which is well-directed at the target set up by the editors.

Professor Sneath has been permitted two bites of the cherry with separate chapters on "Classification of Microorganisms" and "Identification of Microorganisms". Both these articles are at an advanced level inappropriate for this collection. They would in fact have fitted in much more comfortably in *Companion*. Professor H. Smith's chapter on "Determinants of Microbial Pathogenicity" is a good summary of the field and its future prospects but is necessarily rather superficial. Some line drawings, diagrams or photographs would have added to its impact. The rather more specialised topics, "Interactions between phage and bacteria" (Kay), "Resistant forms" (Slepecky), "Symbiosis in the microbial world" (Smith) and "Bacterial nutrition" (Whittenbury) will find interested readers at several levels.

As the individual contributions to *Essays* were written to be available separately some duplication of material was inevitable and even desirable. Nevertheless there is too much of it. Some authors show an awareness of the content of the contributions of others, but in other cases there is needless repetition. In contrast there is little duplication in *Companion* and some useful cross-referencing between chapters. The editors of *Companion* did not impose fixed-length or other formats upon their contributors but the book achieves a remarkable cohesion. The print is small but very clear and the quality of the illustrations and photographs excellent. The nineteen topics chosen reflect to some extent the idiosyncracies of the editors (or those of their friends) but the book is none the worse for that. The topics range wide and most are of direct relevance to contemporary microbiology courses. It is a pity therefore, that they too cannot be purchased as separate offprints by students. However, I feel much more confident in recommending the whole of this book as a cohesive collection than I do about *Essays*.

At £13.50 *Essays* is rather poor value as the articles are of variable quality and aimed at too many targets, but I do recommend teachers and students to look it over and purchase the offprints which interest them. Best buys are Stanier, Richmond, Clarke, Watson and Slepecky. At the lower price of £12 *Companion* is excellent value. The size and format allow for the text to be approximately twice the length of that of *Essays* and the editors and publishers have turned out an attractive authoritative book.

G. E. Mathison

G. E. Mathison is Lecturer in the Department of Microbiology at Queen Elizabeth College, University of London, UK.

Microbiological dictionary

Dictionary of Microbiology. By P. Singleton and D. Sainsbury. Pp. 481. (Wiley: Chichester, UK, and New York, 1978.) £17.50.

It seems to me that two sorts of useful dictionary could be compiled for microbiology. The truly comprehensive dictionary would be a large scholarly work and probably could only be written by five or six people. But to have a single reference work for the whole terminology of this subject (stretching from phytoplankton ecology to the molecular biology of animal viruses) would be of enormous value. The second sort of dictionary is not comprehensive and is of little use to the professional microbiologist but can be of great value to those who lack a microbiological background yet who need to know about some aspects of microbiology in association with another main interest.

The authors' preface indicates that this dictionary is intended for undergraduates and postgraduates in microbiology but I think it is only likely to find a place in my second category. Microbiology students would find little

in the dictionary which was not covered at least as well by their textbooks, even if they have only a very small collection. The complete absence of any references to source of information or to more detailed literature would also reduce the use which could be made of the contents.

The authors have set out to cover "pure and applied microbiology, biochemistry, immunology, genetics and . . . microbiological aspects of allied subjects such as medicine, veterinary science and plant pathology". This is probably too wide a range of material to encompass in less than 500 pages and has led to omissions. Although I was not surprised that there is no entry for *Sterigmatomyces*, it is unfortunate that for instance *Trichoderma* and *Dunaliella* are not included; that is, the choice of organisms has been rather arbitrary, with some very obscure genera included and some important or commonly used ones left out. Comparable examples are the absence of bacteriophages μ and P1, which have an important place in microbial genetics.

The coverage of biochemical terms may equally be criticised for the lack of entries concerning microbial products that are of interest in a wider context, such as pronase, subtilisin and taka diastase. A much fuller account of restriction endonucleases would also have been a timely improvement.

The cross reference of one entry to another is not thorough. For instance, although the entry "red tides" attributes this phenomenon to the alga *Gonyaulax*, the entry "*Gonyaulax*"

makes no mention of red tides. Similarly, although the Helmstetter-Cooper model of the bacterial chromosome replication cycle is described at some length under "growth (bacterial)", the entry "Cell cycle" does not refer to it. Indeed, the entry for "Cell cycle" gave me the clear impression that this term is only used with reference to eukaryotic (usually mammalian) cells, which is misleading.

Coverage of growth physiology and fermentation is poor, with a wide variety of omissions. There is no mention of substrate-accelerated death, maintenance energy or fed-batch culture, which are the kind of terms which non-microbiologists are most likely to seek for definition in a subject dictionary of this sort. The description of continuous culture is very sketchy.

An appendix of metabolic pathway schemes is included, which is a laudable idea, but again the selection is not beyond criticism. Why the tricarboxylic acid cycle is in this appendix whereas the reductive pentose phosphate cycle is in the main body of the text (under "Photosynthesis") is not obvious.

Recognising the limitations of space I feel that many will share the view that too much space has been devoted to material which can easily be found in the literature of biochemistry, genetics and general biology at the expense of many microbiological terms and ideas which could so usefully have been included. **Christopher F. Thurston**

Christopher F. Thurston is Lecturer in Microbiology at Queen Elizabeth College, University of London, UK.

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Basic phycology

IN the introduction to *Introductory Phycology* (Wiley: Chichester, UK; £12.25), F. R. Trainor stresses the importance of consulting more specific works in order to learn how new information is added (to the science) and older facts and interpretations are modified or discarded. Such an outlook was clearly foremost in the author's mind when writing this book for throughout the book the different subjects are suitably placed in their historical perspective and questions are raised in areas deserving of attention in a way that a student might identify with the specialist researcher. The book is perhaps suitable not so much as a reference text but to be read straight through as an introduction to the subject. As such it is well conceived. The author succeeds in conveying his interest and enthusiasm with style and the layout has obviously been

given a good deal of consideration.

Following two introductory sections each of the algal groups is considered separately in twelve chapters, which should give the student a fairly comprehensive account of algal forms. In each of these Trainor deals with the morphological organisation, the characterisation and the reproduction of the class before considering representative genera and other features of special interest. The classification of each group has been deliberately kept to a minimum. Although this is laudable in many respects it might serve to confuse a student particularly in a group like the Chlorophyceae.

The succeeding chapters deal with algal habitats and distribution, classification and morphological variability of benthic organisms, the plankton and their environment, algal control, their usefulness and harmful effects, and finally their culture. In the course of these a good deal of interest is generated and many aspects are treated in

some detail, for example, the section concerning algal products. In the ecological sections the remarks on freshwater and marine environments are nicely balanced and indeed the algae discussed will be familiar to or soon encountered by students in the UK. *Introductory Phycology* is nicely produced and surely a well worthwhile investment for the would-be phycologist.

Illustrations are plentiful both as line drawings and half-tones, a few of which have their limitation. In this respect the book suffers a little by comparison with H. C. Bold and M. J. Wynne's *Introduction to the Algae* (Prentice-Hall: Hemel Hempstead, UK; £16.80), a work which is also a little more detailed in the taxonomic section but which lacks the very interesting general chapters of Trainor. Bold and Wynne's book is subtitled *Structure and Reproduction* and in the preface they declare that the reason for this emphasis is the existence of suitable treatments of algal physiology, biochemistry and genetics elsewhere. It is surprising therefore that the book lacks much ecological information, considering the fact that no equivalent text on algal ecology has been published recently. As an introduction it may for this reason fail to present the algae in their proper biological perspective.

Following a detailed introduction there are nine chapters devoted to the different algal taxa, followed by an appendix dealing with algal cultivation in the laboratory. Sadly Bold and Wynne do not consider whether the prokaryotic 'Cyanochloronta' should be included among the otherwise totally eukaryotic algae, although it would take courage to exclude the group from an algal textbook. However, this approach is consistent with the rest of the book in which the authors have lumped together taxa which would be kept apart by many other workers. For example the inclusion of the prasinoflagellates within the Volvocales may surprise some students and the grouping of the Chrysophyceae, Prymnesiophyceae, Xanthophyceae, Eustigmatophyceae, Bacillariophyceae and Chloromonadophyceae in the Chrysophycophyta on the grounds of similarities in storage products and general pigment composition will be regarded as a retrograde step by many phycologists.

Each chapter consists of general information at and above the ordinal level and a more detailed treatment of representative genera. Regular inclusion of dichotomous keys will help the student to recognise characters of diagnostic (and taxonomic) importance provided it is appreciated that the keys

are not comprehensive. It is a pity, however, that this format is not adhered to in all the sections, for example, those concerning the Cryptophycophyta and the Bacillariophyceae. The treatment of the diatoms is particularly disappointing and only about 20 pages are devoted to this group compared with 33 pages for the dinoflagellates and 186 for the Chlorophycophyta. Admittedly there is not a great deal of variation in basic diatom morphology but no genera are dealt with in detail and there is not one good illustration of a raphe.

Introduction to the Algae has as

much detail available to him as the would-be phycologist should require concerning structure and reproduction; this with the help of copious excellent illustrations and up to date references (though in the latter respect no more so than *Introductory Phycology* by Trainor). The book is beautifully produced and is a good investment, albeit more as a reference text than as an introduction.

R. M. Crawford

R. M. Crawford is Lecturer in the Department of Botany at the University of Bristol, UK.

Ecology textbooks revived

SINCE the population explosion of ecology textbooks some five or six years ago, there has been something of a lull in the field. Presumably the market has been saturated and the plethora of books then produced have entered the cut and thrust struggle for existence. During this time, book prices have risen sharply, and many of the texts by such authors as Ricklefs, Colinaux, Krebs, McNaughton and Wolf have passed beyond a level where the undergraduate is likely to be able to afford them. As a result, my impression is that such authors as Odum and Kormondy have risen like a phoenix from the ashes to re-establish themselves, as a consequence of their low price. Some new invaders in the field are reviewed here.

One of the current releases in Britain is R. E. Ricklefs' *The Economy of Nature* (Cheron. Oregon: £6.75). This, in fact, made an earlier appearance in 1976, but with Blackwells taking over its British distribution, it is more likely to make an impact on its second invasion. It was designed to bridge the gulf between the lengthy ecological tomes and the brief, introductory paperbacks. It is lucidly written, well produced and illustrated, and covers a very broad field from soils and plant geography to environmental physiology, population dynamics and community energetics. Refreshingly, it steers clear of the 'environmental crisis' syndrome. It is essentially very basic and descriptive in its aims. It avoids numerical data; even population dynamics are discussed without reference to mathematical formulae. In my opinion this is a disadvantage, but it may prove a useful way into ecology for those without a scientific background such as some students in the

social sciences, and some of those taking such courses as environmental studies may appreciate its simple approach.

Serious, potential ecologists I would direct elsewhere, possibly to Ricklefs' larger book *Ecology* (Nelson: Nashville, 1973).

J. L. Richardson's *Dimensions of Ecology* (Williams and Wilkins: Baltimore) also starts from a very basic level and avoids numbers wherever possible. Rather than attempting the very broad coverage of Ricklefs, Richardson picks out certain habitats and deals with these in some detail—namely, deciduous forest, fresh water habitats and deserts. Unfortunately, from a European point of view, these chapters have a very parochial, North American flavour. No doubt they will be far more valuable on that side of the Atlantic. The production quality of this book is not what it might be; many figures are very poorly produced. The style is extremely informal, even chatty, to which I have no objection, except where it leads to inaccuracy. For example, when discussing the influence of succession upon biogeochemical cycling in forests, the author states that "we see ecosystems performing excellent plumbing jobs on themselves, repairing leaks in an efficient and seemingly co-ordinated fashion". Apart from the garish anthropomorphism of this statement, it implies (as does its full context) that the "leaks" are fully "repaired" at the climax. This, of course, is not the case, for nutrient discharge must increase as ecosystem growth (and nutrient demand) diminishes.

R. S. De Santo's *Concepts of Applied Ecology* (Springer: Berlin and New York; DM28, \$14) is another attempt at a basic, introductory text. It has certain unique features some of which could have been more fully developed with profit, such as the section on animal and plant physiology. A large portion of the book is given over to terrestrial, freshwater and marine

habitats, but the most distinctive chapters are those dealing with the construction of an Environmental Impact Statement and with toxic compounds in the environment. The former, in particular has an emphasis on the practical issues of ecological survey, which undoubtedly stem from the author's personal experience. The other two unique features consist of a table of conversion factors 28 pages long (it even contains leagues to miles, and kilderkins to metres), and a glossary of 89 pages, which could be regarded as a dictionary of ecology. The latter is reasonably extensive in some areas, but others (for example geomorphology and soils) are not adequately covered. Some definitions are far too vague, for example marsh, bog and fen appear synonymous ('swamp', it seems, is dominated by trees).

Finally, we have two new plant geography texts. The bulk of R. Daubenmire's *Plant Geography* (Academic: New York and London: \$21.50, £15.25) consists of descriptions of plant communities in relation to climate and soils, with an almost complete concentration on North America. Only in the tropical section does Daubenmire step beyond the United States to any significant degree, and here the emphasis is mainly upon Mexico. Within these geographical constraints, however, the author has produced an interesting and informative book. It is obviously aimed at giving the North American student an understanding of his country's natural vegetation. Introductory chapters set the scene in terms of plant dispersal and limits and also provide a condensed account of the history of North American vegetation. It fills a gap in the literature and will undoubtedly be welcome in the USA.

A. S. Collinson's *Introduction to World Vegetation* (Allen and Unwin: London; £6.50) has a somewhat misleading title, for the bulk of it is concerned with general plant ecology, and only the last one third is devoted to the subject of the title, *sensu stricto*. All of the topics dealt with in the first part are treated more fully and more effectively in a variety of other texts, but the second part makes a real and useful contribution to the undergraduate literature. It deals with the world's major vegetation formations, but instead of listing plants, as so many texts do, it collates much interesting ecological information derived from current literature. Such features as soils, variations in vegetation with topography, productivity, and nutrient cycling processes, receive some prominence in Collinson's treatment. It is a shame that more space in the book was not given to this section and less

to the rather superficial account of plant ecology.

It is probably a semantic impossibility to define a vacant niche (in the Hutchinsonian sense), but there does seem still to be a vacancy in the ecology textbook scene for a low-priced, brief, and up-to-date introduction to the subject from a biological point of

view. Odum's *Ecology* (Holt, Rinehart and Winston) and Kormondy's *Concepts of Ecology* (Prentice-Hall) still hold their own in this sphere, but this may in part be due to lack of competitive challenge.

Peter D. Moore

Peter D. Moore is Senior Lecturer in Plant Sciences at King's College, University of London, UK.

Population ecology

ECOLOGISTS, in seeking to explain the distribution and abundance of organisms, must be versed in a wide range of skills that embrace both theory and observations made in the laboratory and field. As in most other branches of science, they must understand the principles upon which their subject is founded and must know how to collect and analyse useful information. Sometimes this information is further used in the development of mathematical models, and all ecologists should be at least aware of the aims and limitations of modelling in ecology. The complete ecologist's library thus includes works on the principles of population and community ecology, on sampling, statistics and the techniques of analysis. The quartet of books to be reviewed here conveniently spans this range.

There is now an abundance of general textbooks of ecology, which usually encompass much the same material treated in similar ways. *An Introduction to Population Ecology*, by G. Evelyn Hutchinson (Yale University Press: New Haven and London; £12.60) does not fall into this category. It is a personal view of the subject that will delight and stimulate ecologists from the undergraduate level onwards. The principles underlying population growth, mortality, natality, competition and the niche are all presented with masterful clarity as befits one of the founders of modern ecology. But this is no abstract discussion of ecological theory. The wealth of examples from both animals and plants place the emphasis firmly in the real world, and at the same time serve to show how valuable can be the basic tenets of ecology when properly wielded. The flaws lie in what is not included. Predation is as important an ecological process as competition and yet is quite inadequately treated in five pages. As disturbing in an 'introductory' text, is the complete absence of any discussion of dispersal, migration and the general importance of spatial heterogeneity. But what singles this book out more than anything else for me, and will I think endear it most to its readers, is

the mine of historical information that runs parallel with the main text in 388 footnotes. Many of these are just bibliographical references, but others are essays in themselves—such as that on the origins of ideas on territoriality that occupies over two full pages of text, and the delightful cameo portraits of William of Ockham, Malthus, Verhulst and many others. The material in these footnotes is wisely included in the main index which leads to such unlikely entries as "Hortense, Queen", "Josephine, Empress" and "Mussolini, B."

The second book in this quartet, *Guide to the Study of Animal Populations*, by James T. Tanner (University of Tennessee Press: Knoxville; \$8.95,) makes a good companion to the first. It deals with the methodology underpinning the study of populations, with chapters on sampling and estimating density, sex and age composition, population growth, mortality and survival. For the first time since 1966 when T. R. E. Southwood's book on *Ecological Methods* first appeared (Methuen; and now extensively revised in a second edition by Chapman and Hall), there is a companion on the same general subject. At about one quarter the length (but only about half the price) and with a very much smaller bibliography, it can be no substitute for Southwood's classic work. It is, however, very clearly written and, by the use of carefully worked examples, presents the analytical techniques such that they may be easily understood and applied. There is for instance a very clear treatment on the derivation of instantaneous birth and death rates and how they may be estimated from data. The book concludes with a useful chapter on the mathematical relationship between population parameters, followed by a short compendium of some different kinds of population model.

A more detailed treatment of mathematical models is to be found in *An Introduction to Systems Analysis: With Ecological Applications*, by John N. R. Jeffers (Edward Arnold: London; hardback £10; paperback £5). This book explains well the objectives of systems analysis and guides the reader through the tangle of terms for

different kinds of model. There are deterministic and stochastic models, descriptive and predictive models, dynamic models, compartment models, matrix models, multivariate models, optimisation models, game theory and catastrophe theory models, all illustrated with particular examples. For a brief flavour of these, I can think of no better text. My main criticisms are aimed at the ten tables with computer listings. These convey little useful information and the space could have been more profitably used.

The final book in this selection, *Statistics*, by D. Freedman, R. Pisani and R. Purves (W. W. Norton: New York; \$13.95, including an *Instructor's Manual*), is a general statistics text, and not aimed especially at biologists. It fits into this quartet only in that a good grounding in statistics is important for any ecologist. The book is intended for "nonstatisticians who want to learn some statistics in order to go about their affairs", and relies on the plentiful use of exercises and a smooth narrative. The authors' have made no

concessions at all to what they call "pluginski", the rather mindless application of a statistical technique to a problem, and have whenever possible banished equations from the text. The result approaches 600 pages of text and notes with an almost complete absence of the different computational recipes needed in the application of everyday statistics. This over-reaction, together with the neglect of any treatment of the Poisson and negative binomial distributions that are so important in ecology, leaves a book ill-suited to the ecologically minded student.

For a concluding remark, I return to Hutchinson's *An Introduction to Population Ecology*. It stands apart from the rest as a work of distinction, that should be studied for its scientific content and savoured for its recondite footnotes.

M. P. Hassell

M. P. Hassell is Reader in Insect Ecology in the Department of Zoology and Applied Entomology at Imperial College, University of London, UK.

Sociobiological literature reviewed

THE three books reviewed here all reflect the recent growth in sociobiology. They are, nevertheless, very different in character. *Readings in Sociobiology* and *The Sociobiology Debate: Readings on Ethical and Scientific Issues* are collections of reprinted papers (there also are some new articles in the latter volume), while *Sex, Evolution and Behavior* is a new textbook written at a level suitable for first- and second-year undergraduates.

The contrast between the two sets of readings is graphically demonstrated by the fact that only two out of 60 articles in the two are identical: Wynne-Edwards on group selection and Trivers on reciprocal altruism. *Readings in Sociobiology* (Freeman: San Francisco and London; hardback \$18, £9.50; paperback \$9, £4.50), edited by T. H. Clutton-Brock and P. H. Harvey, is aimed at undergraduate or graduate student audiences, and it covers the subject of sociobiology as understood by most academics working in the field. Concepts such as inclusive fitness, evolutionarily stable strategy, parental investment, and cost-benefit ratio crop up on every other page. *The Sociobiology Debate* (Harper and Row: New York and London; \$12.95, £7.95), edited by A. L. Caplan, is largely about the subject as

seen through the eyes of the popular press, politically motivated scientists, and some philosophers of science. Biological determinism, sociobiological theory, pluralism, and Science for the People are phrases which give an idea of its contents.

Both books have their place. The collection edited by Clutton-Brock and Harvey is well chosen and a fair representation of the current state of the field. Important subjects such as altruism, sexual selection, game theory and comparative socioecology are included; and Trivers, Hamilton and Maynard Smith between them wrote half the reprinted papers. There are brief and pithy introductions by the editors to each of the four sections. The collection reflects the bias in the sociobiological literature as a whole towards theory at the expense of data. Only three out of 18 papers are specifically concerned with testing ideas (Bertram on lions, Packer on reciprocal altruism, Clutton-Brock and Harvey on primate socioecology); and no experimental work is included. The book should be useful as an adjunct student text in places where the original journals are not available.

Caplan's book is, I assume, aimed at a different audience, the nature of which is not clear. The papers are in five groups: older writings of T. H. Huxley, Kropotkin and others; 1960s 'human ethology' (for example, Lorenz); philosophers comments; the biology of sociobiology (Hamilton, and others); and two long sections on the polemics arising out of E. O. Wilson's book. These last two

sections are the core of the collection. There is probably some value in a number of book reviews, short comments, and the Wilson versus Science for the People exchanges collected together; however, the overall result has little coherence. Editorial summaries might have helped, but Caplan's own contributions are sometimes confusing in themselves. This is illustrated by an amusing juxtaposition at the start of the book. E. O. Wilson writes in his Preface "Sociobiology is a discipline, not a particular theory". Two pages later, Caplan starts his introduction by saying of sociobiology "The very boldness of the theory . . .".

M. Daly and M. Wilson's clearly written book (*Sex Evolution and Behavior*; Duxbury Press; North Scituate, Massachusetts) does more to disperse the clouds of confusion, both deliberate and unintentional, about what sociobiologists have said, in particular the little they have said about human behaviour. After a brief introduction to modern ideas about natural selection, the book races through the cost of sex, sexual selection, parental investment, mating systems, life-history strategies, the physiology and ontogeny of sex differences and finally human sexual behaviour. The style of writing is direct and easy to follow; the contents accurate within the constraints of an elementary treatment. A wide range of literature including evolutionary ecology, anthropology, physiology, psychology and sociology is discussed.

Daly and Wilson are strong advocates of the view that sociobiology has a lot to say about human behaviour. Those who think that this is an overenthusiastic standpoint will at least find that Daly and Wilson summarise their ideas with clarity. I quote from their last chapter. In discussing a statement of the Science for the People Sociobiology Study Group they say "Consider the following excerpt from a statement recently signed by thirty-five authors, some of them well-known scientists:

... determinists assert that the possibility of change in social institutions is limited by biological constraints on individuals. But we know of no relevant constraints placed on social processes by human biology. There is no evidence from ethnography, archaeology or history that would enable us to circumscribe the limits of possible human social organisation."

What can this mean? If human gestation, infantile helplessness, language capacity, sensory systems, cognitive abilities, maturational rates, senescence, and so on, are not "relevant constraints placed on social processes by human biology" whatever could be?"

John R. Krebs

John R. Krebs is Lecturer in Zoology at the Edward Grey Institute of Field Ornithology, University of Oxford, UK.

Marine science

An Introduction to Marine Science. By P. S. Meadows and J. I. Campbell. Pp. 176. (Blackie: Glasgow and London, 1978.) Paperback £5.25.

THIS is the latest addition to Blackie's *Tertiary Level Biology* series of textbooks designed for course options at advanced undergraduate level in universities and polytechnics. The word *Introduction* in the title should be interpreted in this context: the book assumes that its readers possess a good biological knowledge and a reasonable background in physical science. Advanced school students, first-year undergraduates and interested non-specialists would find R. V. Tait's *Elements of Marine Ecology* (Butterworths: London, second edition, 1972) an easier and more congenial first book on marine science.

Meadows and Campbell's book covers a wide field, both geographically and scientifically. Consideration of the marine environment ranges from the poles to the tropics and from abyssal depths to mangrove swamps. There is a very short introduction indicating the extent and characteristics of the oceans and of the ecological zones (with their associated biotic communities) that are described in later chapters. This is followed by a description of water circulation and movement, and the more important physical and chemical processes that occur in the sea. Intertidal regions, estuaries, the sea floor and tropical inshore regions are then treated in some detail with respect to geomorphology, ecological significance and interactions between the plants and animals that live there. Collecting gear and methods of investigation are described and illustrated. There is a special chapter on "Productivity, Plankton and the Pelagic Environment" and another on fisheries and farming. A good bibliography has been classified into reference books, popular books, marine review journals, abstracts, and research journals concerned with marine science. There is a comprehensive index with useful cross references.

The style of the book is very terse. A great deal of information has been packed into a short space by making the text and illustrations interdependent rather than complementary; imaginatively conceived diagrammatic presentation of facts and concepts is used to save more lengthy explanations. In consequence both text and illustrations require close attention from the student. Some unwelcome

effects of compression are evident: the book is rather uneven, physiological considerations receive scant attention and some sections—for example, that on fisheries—are too short to give a representative view.

I believe that this textbook will be a valuable adjunct to taught courses in marine science; it will give serious

students a good follow-up to lectures, and guide and encourage further reading. It deserves a place in libraries and on student bookshelves in many countries.

A. B. Bowers

A. B. Bowers is Senior Lecturer in the Department of Marine Biology, University of Liverpool, UK.

Freshwater biology

Biology of Fresh Waters. By P. S. Maitland. (Blackie: Glasgow and London, 1978.) Hardback £11.50; paperback £5.75.

Biology of Fresh Waters is one of a number of titles in a series called *Tertiary Level Biology*, which covers selected areas of biology at advanced undergraduate level. Although designed specifically for course options at this level within universities and polytechnics, the series will be of great value to specialists and research workers in other fields who require a knowledge of the essentials of a subject.

There are few, if any, books dealing with this subject in a comprehensive manner for undergraduates; this book is therefore a welcome addition to the list of available textbooks for biology students. The author has had considerable experience in both teaching and research in this field and has put this to good use in compiling this work.

After dealing with the physical, chemical and biological characteristics of the freshwater environment he introduces the reader to the plant and animal kingdom in a most thorough and concise manner, which is of great value to the student of both biology and ecology. In these days of early specialisation few students get the necessary basic grounding in plant and animal taxonomy and morphology, and the author has appreciated this in providing a 35-page summary on this topic.

Having provided the foundation to the subject Dr Maitland goes on to consider standing and running waters; here, there is inevitable repetition from

chapter 1. In this respect it is difficult to know whether or not the book has necessarily been produced with the chapters in a sequence to avoid repetition and produce a smooth continuity. The remaining chapters are: field studies; adaptation to environment; communities and energy flow; and fresh water and man—all admirable topics but not necessarily introduced to the reader in the right order.

Although freshwater biology has been the subject of many books designed for various levels of readership, quite often the same illustrations appear with a monotonous regularity. It is therefore refreshing to see new, and in some cases original, figures, diagrams and tables, and the author has used some of the results of his own research on the River Endrick and Loch Leven with good effect. The drawings of plants and animals are particularly good but those illustrating various types of equipment are poor (Fig. 5.3, 5.4, and 8.2) and in some cases naive (Fig. 5.15) or unnecessary (Fig. 8.5).

Man's influence on the freshwater environment has been given a wide, though uneven, coverage. Although this subject must necessarily be brief, a few more references to further reading could have been given to the reader who had an interest in any of the subjects covered, which include fish-farming, hydro-electricity, navigation, pollution, recreation and agriculture.

Dr Maitland should be congratulated on producing a very valuable student handbook on freshwater biology which will be of use to students both in the British Isles and abroad.

Derek Mills

Derek Mills is Senior Lecturer in freshwater ecology and fisheries management at the University of Edinburgh, UK.

The complete biologist

EVEN within a course the best teachers do not rigidly adhere to a recommended textbook. Although such bibliolatry would be an anathema, the choice of student texts is an important, even crucial process. What are the criteria?

Factual content, conceptual approach, readability, layout, production and cost rank among the more important. And for the general biology college teacher, the balance is particularly difficult to find. The three books reviewed here cover much of the same ground at a similar academic level; they are all well written; and they are all intended for a primarily American audience (although Hardin

and Bajema tend to use less idiosyncratic field examples, and might therefore be a more acceptable text in the UK). The contents are ordered slightly differently, but the major topics of modern biology (molecular biology, cellular biology, physiology, ethology, ecology, evolution and genetics) form the basis for each text.

The books reflect different underlying philosophies. G. B. Moment and H. B. Habermann in *Mainstreams of Biology* (Williams and Wilkins: Baltimore) believe that an historical perspective gives "a lasting sense of biology as an ongoing human enterprise", but they've gone overboard. No student should be expected to wade through the names of eight or ten scientists (and their institutions) on a page of text. Similarly, the book is liberally illustrated by photographs of the illustrious. (It's true, I didn't know that Lynn Margulis wearing dark glasses and a headscarf bore an uncanny resemblance to Greta Garbo.) B. T. Scheer, in *Patterns of Life* (Harper's College Press: New York and London) is, if anything, prone to the other extreme. He is clearly a good, experienced teacher who knows how to communicate to his students, but his book lacks lustre in both conception and production. The dedicated student might do well with the somewhat stodgy approach. The average student would not. In Scheer's defence, however, goal questions are effectively set at the beginning of each chapter and brief introductions at the start of each of the six sections aid in a sense of achievement. G. Hardin and C. Bajema chart a good middle ground in *Biology: Its Principles and Implications* (Freeman: San Francisco). Their book is excitingly written, clearly and liberally illustrated and beautifully produced. They have thought hard and well about this new edition, and Bajema's academic qualities (of which more later) make a welcome complement to Hardin's hard-hitting evangelism.

But what of content? Perhaps this is best treated at two levels. First, the balance that the authors give to the various components of biology. Second, the extent to which they are in touch with new ideas and research, and have the ability to integrate them into a student text. Ignoring the books' excesses for the moment, I found Scheer extremely weak on molecular biology. It is hardly integrated into the text, but almost totally confined to an Appendix entitled "The Chemical Explanation". That's not good enough and, as a consequence, topics such as photosynthesis, hormone action and nutrition are very poorly dealt with. Similarly, Hardin and Bajema are relatively weak on molecular and hormonal biology. Perhaps because they want to excite their audience, they gloss over some of the things students ought to learn—straight ethology, for example. If

students are going to progress beyond the level at which these books are aimed, or if they are going to develop an amateur interest in Natural History, then taxonomy will be of importance. It is here that two of the three texts are inadequate. Moment and Habermann barely mention the topic, whereas I found Hardin and Bajema's treatment over-technical and unexciting. I know that taxonomy is not usually exciting, but Scheer makes a valiant (90-page) attempt with a clear phylogenetic exposition. He deserves credit. The books each had so much ground to cover that they rarely deal with topics in excess, though Hardin, not surprisingly, is one for the eco-crisis.

These books are generally up-to-date, using new material which has been largely condensed from specialist sources. It is here that Hardin and Bajema come right to the fore. Not only have they assimilated current research from each area that they cover, but (within my own specialities at least) they have also managed to describe the more exciting and potentially important findings. At the

end of each chapter, they refer to *Scientific American* offprints and other easily conceptually accessible readings. And they haven't missed the human angle. In "boxes" scattered through the text are reprints of short, classic papers from Darwin to Watson and Crick (including, of course, *The Tragedy of the Commons*—but that's excusable), asides on concept and method, and even political analyses. This text, alone among the three, is integrated at almost every level, has set problems (some of which are tersely answered at the back) and is a mine of accurate well-presented information. With the few reservations outlined above, I commend it for serious consideration even by those who already have a favourite text. Everyone, like me, will learn something new from the book.

Paul Harvey

Paul Harvey is a Lecturer in Biology at the University of Sussex, and currently a visiting faculty member in Biology and Anthropology at Harvard University, Cambridge, Massachusetts.

Comparative physiology

In *The Vertebrates: Their Forms and Functions* (Charles C. Thomas: Springfield, Illinois; \$15), C. G. Crispins attempts to survey the whole of vertebrate zoology. Unfortunately, by the time he has covered histology and phylogeny in outline, and named and subdivided the skeletal, muscular, lymphoid, nervous, integumentary, respiratory, digestive, excretory, reproductive and endocrine systems he has virtually no space left actually to describe them, let alone discuss their functions. There is little to be said in favour of this relatively expensive book and it is hard to see for whom it could be useful. The blurb on the cover refers to it as "an alternative approach to contemporary vertebrate zoology" and so it is, by being about 50 years out of date. It is the worst sort of students' cramming book, giving more emphasis to artificial subdivisions and their names than to functional significance, and one can imagine them learning by heart the three names of the methods of tooth attachment to the jaw in order to pass a multiple-choice test. In spite of lip-service paid to function it is essentially morphological in outlook and leaves the reader with a false sense of what should be a living and exciting

subject. It is out of tune with modern ideas and contains just the sort of factual information best calculated to put advanced school biologists off their subject (and it is too elementary for any later course).

Older British readers should not be put off by the slangy title of *Mammalogy* (W. B. Saunders: Philadelphia and London; £12) by T. A. Vaughan, as it can be thoroughly recommended as a very satisfactory reference book for second- or third-year undergraduate zoologists onwards. Probably nowadays few university courses are so specialised on mammals that it could be a major required textbook, but for any course specialising in vertebrates it would be one of the first books in the library to refer a student to when a question arose concerning mammals.

After a brief description of mammalian characteristics, the evolution of mammals from the mammal-like reptiles is surveyed. The whole complex story of the therapsid radiation cannot be told, but sufficient description (including diagrams) of key types such as *Cynognathus* is provided to give a fair account. The remainder of the first half of the book deals with the various orders in turn, giving detail to family level, and stressing ways of life as well as standard morphological detail. On the whole, the less well known groups are described more fully.

and in particular that miscellaneous collection of animals masquerading as the order Insectivora is introduced in masterly fashion. There are photographs of members of nearly every family and clear drawings of skulls, teeth, limbs and other skeletal details as appropriate, all portrayed in a clear, uniform style similar to that first adopted in Romer's *Vertebrate Palaeontology*.

The second half of the book is devoted to a number of topics of general biological interest which have special relevance to mammals: ecology, zoogeography, behaviour (especially social behaviour), reproduction, metabolism and temperature regulation and echolocation, with a final brief comment on the effects of man on mammalian wild life. From this list it can be seen that only certain subjects are included, but many of them are of obvious importance like the section on zoogeography. This covers a summary of types found in the various regions, a description of continental drift and its effects including a very useful discussion of the "unusual" faunas, past and present, of South America, Australia and Madagascar. Other sections like that on echolocation in bats and cetaceans provide concise summaries of detailed topics which presumably reflect the author's interests.

It is very easy to read large sections of the book straight through, as well as to use it as a reference book for facts, ideas and as a lead in to further reading. It is well produced and relatively cheap—in fact a good buy.

At first sight one might suppose that *Animal Physiology*, by Roger Eckert and David Randall (Freeman: San Francisco and London; \$19.95, £12.95), is merely another in the series of comparative physiology texts which started with Prosser's and which has been added to at the rate of about one every four years ever since. And so in many ways it is, for much of the content is in common with the latest editions of its rivals. But this attempt does the standard job well, and has many advantages and few disadvantages when its special emphasis is identified. The book starts with basic concepts of physics and chemistry (for example, Ohm's Law and osmosis), works through cellular processes such as the electron transport chain, active transport and membrane potentials, and then covers individual nerve and synaptic functioning. This leads on to sense organs and the principles of central nervous integration, then to the other systems: muscle, cilia and flagella, chemical messengers, osmoregulation and excretion, blood circulation and gas exchange. There are appen-

dices containing useful formulae (though kinetic energy should not be printed as $1/2mv^2$) and a glossary.

The main theme of the book is the explanation of physiological processes and their control; it is not a survey of comparative physiology. The choice of which processes are to be described and explained is rather selective, but mammalian functions are in general fully covered (apart from digestion and temperature regulation) and so are the major variants and additions that occur in other vertebrates and in some invertebrates, for example, osmoregulation and circulatory systems.

In spite of the complexity of the subject the book is readable. Within its chosen limits, it is an excellent textbook, clearly most appropriate for second- and third-year undergraduates taking courses in comparative or in mammalian physiology. It highlights the fact that so many processes in physiology now have their explanations pushed back to more molecular levels than they did 10 years ago. The reader

of this book might be forgiven for thinking that there is little more to be found out, so complete and satisfying are so many of the descriptions given. Rightly, in the cause of clarity, the text contains only sparing use of "probably" or "as far as is known at present" and the topics described tend to be those for which an explanation has been suggested in the literature. So it is the sort of textbook which reinforces and extends a lecture course, rather than taking its place, and it is essentially aimed at helping students understand how animals work. It is intentionally not a reference book, but the references which are given should be quite enough to allow a student (or a lecturer for that matter) to use it as the start of a more detailed library search on any specific topic.

R. W. Murray

R. W. Murray is Senior Lecturer in Zoology and Comparative Physiology at the University of Birmingham, UK.

Fundamental nutrition

Fundamentals of Nutrition. Second edition. By L. E. Lloyd, B. E. McDonald and E. W. Crampton. Pp. 466. (Freeman: San Francisco, 1978.) \$19.50.

THE title of this book gives a very good indication of its contents and the intended readership. Each chapter is short so as not to overface the first-year undergraduate or college student, and at appropriate points boxes are inserted with topical information such as breeding cereals for high lysine. Potted biographies of eminent nutritionists also add to the readability of the main text.

Introductory chapters describing the composition of foods and animal tissues are followed by sections on energy and protein, vitamins and minerals; the latter, sensibly, not illustrated by well-used photographs of animals suffering from various deficiencies or excesses. Photographs could, however, have been usefully added to illustrate some of the more complex methods which are well described in the text.

A further group of chapters includes, refreshingly, discussions on experimental design and analysis and descriptions of the major areas of animal experimentation in nutrition, although

many students would presumably be taking a biometry course in parallel with nutrition and would be referred to a more detailed and comprehensive text in that subject. The same might be said of some of the biochemical coverage; surely no modern nutritionist can escape without at least one full course in biochemistry which would render superfluous those areas dealt with in this book.

The last five chapters, occupying almost 100 pages, deal admirably with quantitative aspects of nutrition—how much of each of the previously defined nutrients are required daily by various species, including man, in various physiological states. This is not to imply that the book is a practical feeding manual, and those examples of farm animal feeding that are given are based on American standards which are not relevant to a large part of the world. The use of the calorie rather than the internationally agreed joule as the unit of energy adds further possible confusion for the majority of students who are now familiar with the latter. There is a useful index.

This is a comprehensive coverage of the whole subject of nutrition and is written in a manner that will be appreciated by all who are tackling this subject for the first time.

J. Michael Forbes

J. Michael Forbes, is Lecturer in Animal Physiology and Nutrition at the University of Leeds, UK.

Plant pathology

THE approach of *Fundamentals of Plant-Pest Control*, by D. A. Roberts (Freeman: San Francisco and London; \$17.50; £10.20) is to try to unify all aspects of plant pest control, pointing out common features as much as possible, and culminating in the necessity for the systems approach in order to achieve integrated pest control. This is a theme of much recent thought on the subject and it is evidently right that undergraduates should study and evaluate the approach.

After a general introduction to the plant-pest ecosystem, a series of chapters reviews the structure and functioning of crop plants, followed by the dynamics of plant pest populations and the basic elements of plant pest control. The second main section reviews different types of pests, in the wide sense: microbial pathogens, nematodes, arthropods, weeds and vertebrates. Finally, the systems approach is outlined and the author concludes with his reflexions on pest control in the future.

Desirable though the ultimate objective may be, one is left somewhat dissatisfied with the book as a student text and not altogether convinced by the arguments as they stand. There is an unsatisfactory discrepancy between the potential readership of the book and the background knowledge one would expect of the reader. If it is intended for the naive reader, who has not studied plant pests in the more traditional way, then it is hard to see how he can properly evaluate the whole of the argument or cope with the use of terms like mycoplasma-like organisms or /2-chloro-4-(ethyl-amino)-6-(isopropylamino)-s-triazine/. If, on the other hand, the reader has the background, then why does he have to be given a diagram showing an elementary view of plant structure, be told that organic compounds contain carbon, or study a table setting out among more appropriate things the type of metamorphosis in stoneflies, book-lice and fleas. Such a book must surely aim to refine the understanding of a student with an adequate background, and in this sense more than a third of the text is rather unnecessary.

Leaving this aside, the sections on each type of plant pest are well structured accounts of the application of basic principles to each case. These chapters, contributed by different specialists working consistently to the author's theme, are, with the exposition of population dynamics and the

basic elements of control in the first section, the strongest section of the book. The plant pathology, entomology or weed biology student, in a context where such people are still separate, as is largely the case in the UK, will most profitably read the chapters which do not relate directly to him. The unifying approach cannot, however, always accommodate the real situation altogether. There is ultimately not so much in common between vertebrate pest control and that of nematodes as there is between the latter and plant disease control. The concepts of vertical and horizontal resistance arising from van der Plank's view of plant pathology and applying to races of host-specific pathogens and varieties of their hosts transpose rather superficially to the interactions between plant and insect species. Nevertheless, detailed differences should not submerge the most important fundamental similarities.

The systems approach of the last section is illustrated by the system "undergraduate education in crop-plant-pest control". Why, one wonders, is it not possible to illustrate it by a real plant protection system? The latter only appears in a highly condensed account of computer models such as EPIDEM. One can accept the validity of computer modelling or integrated pest control as such, but this text does not altogether convince one of the real, productive, value of the "higher systems level", or systems approach itself. Undergraduates may profitably

read this text but should keep their feet firmly on the ground and their critical faculties well in trim.

G. N. Agrios' *Plant Pathology* (Academic: New York and London, \$22.95) was in its first edition a textbook one was only too glad to recommend to students. It was invaluable for its thorough and clearly expressed accounts of selected diseases, for its abundant and excellent illustrations and life-cycle diagrams, and for the confidence with which one could advise students to refer to it as a supplement to lecture courses. It had its faults: the introductory chapters on general concepts were rather solid catalogues of information, without much feel for the essentially research orientation of subjects such as biochemical defence mechanisms; the rather limited number of diseases treated was often a problem, especially for the European user.

This new edition triumphantly corrects the latter fault, and is generally revised and brought up to date. The introductory chapters have been somewhat polished and some superfluous material has been removed. There is no doubt that this new edition is even better than the first and it can be thoroughly recommended as an invaluable teaching aid with very few reservations.

I. M. Smith

I. M. Smith is Deputy Director of the European and Mediterranean Plant Protection Organization in Paris.

Plant anatomy

THE first edition of *Plant Anatomy* (Part I: Cells and Tissues) by E. G. Cutter, was published in 1969 and contained 168 pages. Now, almost ten years later (Edward Arnold: London; hardback £13.50; paperback £6.75), the 315 pages contain considerably revised and updated information while retaining the essential character of the original volume. Considerable use has been made of both scanning and transmission electron micrographs and, in general, the reproduction of these is quite respectable considering the quality of paper and method of printing. It is good to see such comprehensive revision—to the extent that transfer cells warrant a whole new chapter of their own—but it is a pity to find that some other cell types (for example, endodermis and hypodermis) are relatively very neglected. To some

extent this arises because this book is a companion to the second part (Organs) in which the endodermis, for example, is more fully described. Nevertheless, it is regrettable that while, as written in the review of the first edition (*Nature*, 222, 1100; 1969), "No cell types are omitted . . .", the degree of coverage is so variable.

This is very much a plant anatomy book with a difference: constantly probing and questioning and never very far away from the functions of the cells and tissues being described. Thus, the author is not satisfied simply to classify or to catalogue but attempts to show how the specific structures serve their different roles. Glands, and other cells and tissues, are no longer viewed simply as interesting structural aggregates but also as morphological entities that are uniquely adapted to serve their (described) function. Moreover, mature cells are not assumed to appear *de novo* and Dr Cutter takes pains to stress the subject of differentiation, both in a specific chapter where genetic control, totipotency,

polarity, pattern formation and nuclear-cytoplasmic interactions are briefly reviewed and also at appropriate times in the other chapters. This style leads to a book that is very readable and well suited to elementary students as well as to the more experienced botanists who will undoubtedly benefit from it.

Cell contents and cell walls are described briefly but efficiently in consecutive chapters. All the major organelles are reviewed in up-to-date fashion. It is good to see the most recent views on, for example, C_4 chloroplasts and plasmodesmata, although we might have hoped for a more comprehensive assessment of the structure and role of microtubules than the dozen lines allowed for them. Indeed, the whole area of membrane structure and techniques such as freeze-etching is ignored, as is the work on cell wall formation by, for example, Willison and Brown. This is surprising and disappointing in a book that tries to relate structure to function as membranes are of such paramount importance.

It would have been much more useful if the subject coverage in this book had been more evenly distributed. However, taken as a whole, this is an attractive volume that should encourage botanists to think more about how the structure and function of plants are intimately inter-related. Anything that does this is to be welcomed and Dr Cutter has made a notable step in the right direction.

Applied Plant Anatomy (Longman: London and New York, £4.95), by D. F. Cutler, seeks to illustrate the way that a sound knowledge of plant anatomy can be applied to the solution of many important every day problems. It is good, in a textbook of plant anatomy, to see pictures such as that of a papyrus sandal and to read about "wood in archaeology": this does help the subject to come alive. This novel approach leads to a book that is different from most others on elementary plant anatomy and one that is both readable and enjoyable for even the newest student of botany.

The introductory chapter contains brief notes on techniques for both light and electron microscopy. This is followed by chapters on the basic morphology of plants; a good, simple, illustrated glossary; the histology of plant organs; meristems; vascular tissues; adaptive features; flower and fruit; and economic aspects of applied plant anatomy. Each chapter is well illustrated by clear line diagrams and micrographs. The distribution of micrographs is a little uneven and, indeed, there might have been more of them altogether. At the end of each chapter there is a small list of suggested further

reading and, where appropriate, lists indicating where to look to find particular anatomical characters. This is a most valuable addition and covers plants worldwide. The further reading lists are rather too short and often quite dated. Having commented on the usefulness of electron microscopy to the study of plant anatomy, the author then fails to cite a single further reference in this area. Indeed, the four books suggested at the end of chapter one serve as a rather limited coverage of the subject area.

The introductory chapter on techniques is useful but it is a pity that

some of the figures concerning the resolution of microscopes seem to have been muddled: both TEM and SEM resolution are out by a factor of 100! The index is concise and helpful and, indeed, reflects the whole tone of the book, which is to convey a very considerable amount of information in a palatable and easily understandable style.

A. W. Robards

A. W. Robards is Senior Lecturer and Head of the Electron Microscope Laboratory in the Department of Biology at the University of York, UK.

Principles of plant nutrition

Principles of Plant Nutrition. By K. Mengel and E. A. Kirkby. Pp. 593. (International Potash Institute: PO Box 41, CH-3048 Worblaufen-Bern, Switzerland, 1978.)

THIS book is broadly based in accordance with the implications of its title and achieves a commendable integration of soil and plant aspects of nutrition. The partnership of geographically separated authors, who are both practically orientated university teachers, provides a background of wide experience and as shown by the principal bibliography (about 1,400 references) an extensive familiarity with the subject.

Chapters 1 to 6 (totalling 290 pages) describe some general principles. Considerable attention is given to plant-soil relationships; nutrient uptake, translocation and storage; nutrient availability in relation to crop needs; general effects of nutrient deficiencies; and methods of nutrient application and correction. A number of major physiological activities—photosynthesis, nitrogen fixation, nitrogen assimilation, water relations, active transport, nutrient absorption—are described in sufficient depth to provide the necessary background for understanding the role of various elements, although respiration seems less adequately considered.

Chapters 11 to 18 deal separately with each of the familiar essential elements—except chlorine, which is reviewed in three pages in a chapter confined to some possibly beneficial or indirectly essential elements: cobalt, silicon and vanadium. Reviews on separate elements cover soil aspects of their availability, their metabolic roles and deficiency and some toxicity effects on growth. These chapters provide a good general idea of the importance of the elements. These and the other chapters are each concluded with a general reading list to supplement

the numerous specific citations in the text.

Chapter 19 concludes the treatise with examples of several toxic or less familiar elements which are discussed in more general terms and it includes several interesting miscellaneous reports. There are a few minor omissions—for example, regarding the production by certain plants of apparently specific citrate and oxalate metal complexes especially with nickel and chromium; the quite extensive work on tungsten relegated here to a few words on page 149 on tungsten inactivation of nitrate reductase by unspecified means; the original, rigorous and model investigations of Brownell on the absolute need for sodium by *Atriplex vesicaria* (though his later discovery of its possibly general importance in C_4 -type photosynthesis plants is nevertheless noted); the differentiation of apparently two types of malate enzyme; the exciting conclusions of Meish *et al.* regarding the essential role of vanadium in green algae, thought to involve the formation of delta aminolaevulinic acid (though this news may have 'broken' as the book went to press); and finally (though possibly again just too late), the further work of Polacco on the need for nickel in urea metabolism since the first report on urease by Dixon *et al.* briefly cited on page 514.

This quite comprehensive and well documented book will be of value to students and lecturers alike in faculties of agricultural science, botany and biological sciences and should also be welcome in the libraries of agricultural institutes, advisory workers and consultants. It is especially commended for its relevance to practical aspects of crop nutrition and for proper appreciation of the general principles on which practice should be based.

E. J. Hewitt

E. J. Hewitt is Head of the Plant Physiology and Biochemistry Section at Long Ashton Research Station, and Reader in Plant Physiology at the University of Bristol, UK.

Plant taxonomy

Essays in Plant Taxonomy. Edited by H. E. Street. Pp. 304. (Academic: London, New York and San Francisco, 1978.) £12.50; \$24.50.

PROFESSOR Tom Tutin has no rival for the title of *doyen* of British plant taxonomists, and few would question the appropriateness of this *Festschrift* on the occasion of his seventieth birthday, which so nearly coincided with the completion of *Flora Europaea*, his second *magnum opus*. We must here, however, examine the book, not from the standpoint of an admiring friend, but of a student asking what use it will be to him. Symposia are at present, perhaps, an over-popular form of publication: in many of them the thread of supposed common interest which binds the essays together is far too tenuous, and indeed, in the case of some complimentary volumes such as this, completely non-existent. The present work, however, is free from this reproach, for all but one of the essays are relevant to taxonomy on the strictest definition. The editor, who unfortunately did not live to see his work in print, planned the volume well, but part of the credit for its efficient and speedy production must go to Dr C. A. Stace, whose share was greater than his modesty permits him to disclose.

Four of the essays are wide-ranging reviews with no special claim to an original interpretation, and these are the parts of the book which will be of most value alike to the general student and for those who are specialising in taxonomy. Valentine on ecology, Smith on chemistry and Moore on chromosomes, all considered in relation to taxonomy, give judicious summaries of their themes, with well chosen and up-to-date bibliographies; and Richards on the taxonomy of bryophytes gives an excellent account of a field to which most angiosperm taxonomists are complete strangers. Stace on breeding systems and their taxonomic implications also covers a wide field, but with a more individual approach; he ends with a justified *cri de coeur* for a less Procrustean system of infraspecific categories than that laid down by the International Code, but the genius who can produce a system which is at once flexible, unambiguous and reasonably simple has not yet been found.

Somewhat more specialised, but still in the nature of reviews and thus of value to students whose interests lie in the fields concerned, are the essays by

Hawkesworth, who introduces the reader to the peculiar and complex problems posed by the taxonomy of lichens and their fungal constituents; Parker on taxonomic problems in crop plants; and Hawkes on the related theme of conservation of genetic diversity, especially in plants of economic interest.

Cook on the *Hippuris* syndrome (the frequency in aquatic plants of very diverse families of a vegetative structure superficially similar to that of *Hippuris*) belongs rather to the realms of morphogenesis and evolution than taxonomy, but it should generate some constructive head-scratching of benefit even to the elementary student.

Yeo attempts to explain why *Euphrasia*, in spite of a normal breeding system, presents taxonomic problems as intractable as those of many apomictic or autogamous genera. To the reviewer his effort must be classed

as a gallant failure, but the essay is essential reading for anybody working on semiparasitic Scrophulariaceae. The remaining essays (Walters and Richardson on different aspects of endemism, Sneath and Chater on a rather esoteric analysis of dichotomous keys, and Heywood on past and future Flora-writing) are also of interest mainly to the research-worker or teacher.

Almost without exception the essays have a lucid and readable quality which sets them apart from the contents of most biological journals. The book should be dipped into by all honours students of botany. Those whose interests lie mainly in fields close to taxonomy should read most of it carefully and chase up some of the clues given in the excellent bibliographies.

D. A. Webb

D. A. Webb is Professor of Systematic Botany at Trinity College, Dublin.

Pollen analysis illustrated

An Illustrated Guide to Pollen Analysis. By P. D. Moore and J. A. Webb. Pp. 133. (Hodder and Stoughton: London, 1978.) Hardback £8.50; paperback £4.95.

THE authors have produced this book, intended for undergraduates and advanced school students, as a laboratory manual. They hope that it will be used as a supplement to Pennington's *History of British Vegetation*. The book is a guide to pollen-bearing Quaternary deposits, the collection and treatment of samples, pollen morphology, the identification of pollen, the practice of pollen counting and interpretation of results.

Chapter 5 (Pollen and Spore Key with Glossary) will, I am sure, be much used by pollen analysts whether beginners (from school to postgraduate) or established workers. The authors rightly stress more than once the need for a reference collection to corroborate identification made from the key and illustrations. In its liberal supply of plates, including stereoscans, of pollen grains and spores the chapter contrasts strongly and favourably with Faegri and Iversen's famous textbook (*Text-book of Pollen Analysis*) in which the keys are discouraging in their lack of

illustration. The beautifully illustrated, two volume *An Introduction to a Scandinavian Pollen Flora* by Erdtman *et al.* is not easy to come by and is expensive, as are other pollen atlases.

The accounts of important modern developments such as absolute counting, statistical treatments, computing and zonation, are lucid and level-headed, with the right balance of the need for greater objectivity and credit for the many successes of the more traditional methods.

The plates and figures are well executed and appropriate but a reader with little archaeological knowledge could be misled, if temporarily, by Fig. 1.1 which shows the sequence of archaeological periods but gives no indication of timespans. Also I wonder what a beginner will make of the description and drawings (Fig. 3.1 (b)) of the Russian borer, redrawn inaccurately from West's *Pleistocene Geology and Biology*? He may well be puzzled. Perhaps the Mackereth corer, an important though elaborate tool in palaeolimnology, deserves a little more detailed treatment, though few readers will have the chance to use or even see one.

However, these are small points, detracting little or nothing from this valuable, readable book which will be used freely and successfully in the ways the authors intend.

J. H. Dickson

J. W. Dickson is Lecturer in Botany at the University of Glasgow, UK.

Present state of academic psychology

Psychology Survey. No. 1. Edited by B. M. Foss. Pp. 220. (George Allen and Unwin: London, 1978.) Hardback £5.50; paperback £2.50.

WHAT is the perfect introductory text for psychology students? The selection pressures of the market should by now have weeded out the unfit, and we ought to be approaching a form well adapted to the British students' needs. The American form, with its pictures and snippets of information, seems fairly stable, but the comprehensiveness of such products at a shallow level is unsuitable for most British introductory courses. Usually our lectures and tutorials are not part of a single planned course, and tutors, left to devise their own essay titles, naturally look out for up-to-date surveys of currently fashionable topics. The *Annual Review of Psychology* is too heavy and unselective, and the professional review articles, such as those published in *Psychological Bulletin*, involve too drastic a plunge in at the deep end.

It is not surprising, therefore, that there is a market for books of introductory reviews. Early examples were the present editor's *New Horizons in Psychology* (at slightly too elementary a level) and the *British Medical Bulletin's Experimental Psychology* of 1964, followed in 1971 by *Cognitive Psychology*. Recently several publishers have issued series of short paperbacks, each on a special topic within psychology, but these have been uneven in standard; and buying even half a set would be expensive. In the book under review, we have the beginning of what may prove an ideal solution. It is the first of an annual volume of reviews, designed, judging from the contents, to throw the student in just out of his depth. It is, therefore, for the beginner who can rely on personal guidance if necessary, and not for someone setting out on his or her own. Each year, to keep the series fresh, there will be a new editor; and providing we can be kept up to date by suitable recycling of topics, the enterprise deserves to be very successful.

In this volume there are 14 chapters of about 15 pages, each by a different author who is a well known specialist in his field. The titles, in order, are, Visual Selective Attention, Verbal Remembering, Conditioning, Mental Imagery, Psychophysiology, Cognitive Effects of Cortical Lesions, Experimental Child Psychology, Sex Differences, Behaviour Modification, Human

Learning, Cross-Cultural Studies, The Bases of Fluent Reading, Occupational Psychology, and Environmental Psychology. There is thus a gradual shift from the 'core' subjects, highly organised areas currently under tough minded experimental scrutiny, to applied areas of research. In core subjects the reviewer's problem (generally solved remarkably well) is to get across, in a brief and simple form, the involved theories that make sense of the experimental work and that mark the boundaries of the area. Applied areas, on the other hand, are bounded by the set of practical problems that define them, and within such an area different approaches are possible; the result is diffuse, and short reviews inevitably seem selective and sketchy. In all cases,

however, the student is given a start, and a useful list of references.

The reader is left to work out the thread that holds the chapters together. Michael Eysenck, author of *Human Memory*, finds encouragement in "the emergence of general theories that have considerable application outside the limitation of a specific experimental task", but there is not much sign of this in the book and one feels by the end that the areas of psychology, like games, share no more than a family resemblance. That, no doubt, accurately reflects the present state of academic psychology.

A. W. Still

A. W. Still is Lecturer in Psychology at the University of Durham, UK.

Physiological psychology

A NOTABLE feature of this set of books on the central nervous system and behaviour is that each is written by one author, an unusual and welcome event now that so many books consist of collections of previously published papers, accounts of conference proceedings, or chapters by writers whose only common link is an editor. A single author can avoid the well known problems of multi-author works—repetition, abrupt change of style, and the difficulty in developing a point of view by sustained argument. The advantages of going-it-alone certainly show in the present selection, albeit to different extents. In *A Primer of Psychophysiology* (Freeman: San Francisco and Reading; hardback £8.90, paperback £4.40), James Hasset presents a brief, clear and bouncy account of an approach to behaviour that is neglected in most British psychology departments. What is psychophysiology? After rejecting definitions that simply refer to the methods used by psychophysicists, for example, recording the EEG, he settles for "a strategy for studying human behaviour and experience". Now many will argue that the strategy is no more than a reflection of the methods used, and the bulk of the book is certainly devoted to a straightforward description—whose usefulness should not be underestimated—of such things as the electrical properties of the skin, respiration, cardiovascular states, pupillary changes, muscle tone, eye movements, and the gross electrical activity of the brain, together with detailed accounts of how to measure

them. It is undeniable that the measurements correlate well with psychological states such as fear, tension, anger, curiosity, embarrassment, arousal, and habituation, to name only a few, but the reader who is looking for some mechanistic or conceptual explanation of our behaviour will be dissatisfied. Psychophysiology may have useful applications, for example, in the therapeutic use of biofeedback or revealing individual differences and psychiatric conditions, but the book does not show whether it has explained anything important.

On the Texture of Brains (Springer: Berlin; DM16.30, \$7.50), by V. Braitenberg, is equally slim, but there the resemblance ends. It is a collection of eight short essays whose collective message is that "the structure of brains is information about the world", by which the author means not that we can understand the world from brain structure but that the structure of the brain has to reflect certain features of the world in order for the organism to deal with its spatial environment. It is an attempt to understand behaviour in terms of neuroanatomy. A crucial point is that not all levels of anatomical analysis are equally informative. To understand a brain by gross dissection is like trying to understand a text by dividing it into chapters; to analyse it by studying the structure of individual cells is about as helpful as examining prose in terms of the letters of the alphabet. The right approach, it is maintained, is to study how cells are connected. By so doing we find that the basic design is remarkably similar from insects to man. Complexity and variety of behaviour are therefore a product of the total number of cells and their interconnections, rather like the dif-

ference between a short poem and a long novel. It also follows that the general principles of how nerve cells control behaviour may be studied in organisms much simpler than ourselves, which explains why so much of the book is devoted to the author's own leading role in successfully demonstrating how visually guided navigation in flies can be explained by the fibre connections in the eyes and visual ganglia. Now this approach has undoubtedly been fruitful with insects, and promises to be so with the mammalian cerebellum, but major problems arise in considering the cerebral cortex, where input-output relationships fail to reveal what lies between them. It is unclear how learning, memory and prediction are to be dealt with in general terms except by old and loosely formulated notions like cell assemblies, changing patterns of excitation, and internally activated representations that are matched against sensory patterns. How these arise is not explained. And it is easy to see how a slight mistake can lead to serious misconceptions. To take one of many examples, it is stated that the principle of equipotentiality, by which it matters not a jot which part of the cerebral cortex is removed, demonstrates that regional specialisation does not occur. This results in a needless struggle to explain something, for we now know that the principle does not follow from the experiments on which it was based. Nevertheless, Braitenberg has written a joyous, learned and provocative book that makes the reader think at every turn.

Developmental Neurobiology (second edition, Plenum: New York and London), by M. Jacobson, and *Developmental Plasticity of the Brain* (Oxford University Press: Oxford, £4.95), by R. D. Lund, cover similar ground. Both deal with the ontogeny of the nervous system, the differentiation of nerve cells and interactions between them, and the mechanism of neuronal specificity. Apart from price there is not a lot to choose between them, but Jacobson is slightly more detailed and comprehensive, deals more effectively with the neuromuscular system, provides a succinct but excellent account of the extensive work on the retinotectal map in amphibia, and is more lavishly illustrated. Lund is more thorough on environmental influences on central connections and his method of imparting information by asking a question then answering it, instead of simply presenting facts, is helpful, as are his concise summaries at the end of each chapter. If either book lacks anything it is an account of work with invertebrates and some recognition of the importance of behavioural experi-

ments done in conjunction with developmental experiments. But these are slight criticisms of two scholarly works that will be deservedly popular. Their main rival is *The Formation of Nerve Connections* (Academic: London) by R. M. Gaze, which is now several years out of date.

Brain Systems and Psychological Concepts (Wiley: Chichester, UK; £12.50), by J. Boddy, is an undergraduate textbook of physiological psychology, and it is useful to know how it differs from those of similar length and subject matter by Milner, by Thompson, and by Grossman. The overlap with all of them is huge, particularly in describing the sensory pathways, hunger, thirst, sex, emotion, brain stimulation, sleep, memory, and research techniques. However, there is much more emphasis in Boddy's book on the relevance of experiments on human subjects, for example, in considering information processing, and the chapters on mental illness and the effects of drugs are novel. It is much easier to read than either of the two books by Grossman; the arguments are not as thoroughly developed as by Milner; and it is not as detailed as Thompson's book. The overall level of presentation is very similar to the earlier text by Morgan but of course

it is much more up to date. My major reservation is whether we need another textbook of physiological psychology, for the group published between 1967 and 1973 seem adequate as an introduction before the student moves on to more specialised books. But for the student whose reading has to be confined to one book Boddy has provided a sound account with a welcome cognitive flavour.

Who will benefit most from the books? Hassett writes for the undergraduate and his book is unashamedly introductory. Braitenberg will appeal to the widest audience, from student to colleague, because he is such fun and is trying to say something simple and fundamental about the brain; but the reader has to take a lot for granted. The books by Jacobson and Lund are most suitable for advanced students who already know a good deal about the nervous system, and they can hardly fail to become standard reference works for any developmental neurobiologist. Boddy's is the only book that is a student text covering a broad range of topics. It is good, but not clearly better than a number of rivals.

A. Cowey

A. Cowey is Reader in Physiological Psychology at the University of Oxford, UK.

Psychology texts

ONE of the joys of working in psychology is the discipline's apparently unlimited ability to borrow from other areas. At times the joy can turn to anguish when the transplant seems to submerge the host but a new development for "grafting" always seem to come along.

Complications for teaching come from the diverse ways in which departments of psychology seem to split up the subject matter—for example, should there be separate courses on thinking and on psycholinguistics; should both be subsumed within a conglomerate memory, language and thinking; should there be a separate course on artificial intelligence (AI); or should all the preceding courses be collected within one course on cognitive psychology? Possibly a solution lies in the emerging discipline of 'cognitive science'—forming a linkage between linguistics, AI and cognitive psychology. It is against this background of shifting boundaries that course texts for psychology have to be assessed.

Artificial Intelligence (Edinburgh University Press: Edinburgh; £5), edited by

Bundy *et al.*, has more claim to be a course-text than many so-called textbooks. The book derives from lecture notes for the 'Artificial Intelligence 2' course given at the University of Edinburgh (one of the few UK centres where full AI courses are available to psychologists). The '2' is perhaps cautionary—not a course for first-year students; and also it represents a very full year's work.

For those planning their own courses programming exercises through the text, and appendices with examination papers and student response to the course should prove invaluable. Collection of the references throughout the text into a bibliography would have been useful. Readers likely to be disappointed with the book would be those working from a cybernetics: brain - theory: engineering background—the six pages on Perceptrons do not sufficiently reflect work within their area.

Comparison texts would include those by Winston, Boden and the Open University Cognitive Psychology course team. Winston's *Artificial Intelligence* (Addison-Wesley, 1977) is a more technical work with few compromises to non-LISPers. It is really for advanced undergraduates and postgraduates, and is perhaps an AI/computer science text rather than a psychology text. The OU

text relies heavily on access to a terminal tied to the SOLO system (specially developed for this course) and seems a difficult text to grasp in isolation. For many humanities students (including linguists, philosophers, and some psychologists) Margaret Boden's *Artificial Intelligence and Natural Man* (Harvester, 1977) provides a good introduction to the area of AI without becoming too embroiled in techniques. But if your psychology department is lucky enough to have access to a suitable computing system then the Edinburgh text is a good introduction to trying out techniques in AI.

Psycholinguistics (Pentice-Hall: Englewood Cliffs, New Jersey; £10.90) by Foss and Hakes lives up to its subtitle—*An Introduction to the Psychology of Language*. Concentration of psychology rather than linguistic theories or developmental processes produces an excellent overview of the chequered area of psycholinguistics. From a murky mixture of mechanical translation, S-R psychology and information theory, through the revolution of Chomskyan grammar, psycholinguistics has reached its present stage of uncertainty and division. Claims to form a separate discipline seem to be fading; merging into more traditional areas of psychology and supplying part of the backbone of cognitive psychology seems more likely.

The introductory chapters provide a good discussion concerning the nature of language, and anchor the book against a linguistic as well as psychological background. Three chapters then cover 'input operations'. An excellent exposition and discussion of speech perception is followed by a comprehensive treatment of comprehension—indicating other sources of information than pure syntax. The section concludes with an integration of sentence understanding with various models of memory. The next section deals with 'output operations' and is divided

into the planning of sentence production and the production mechanisms involved. There is good treatment of the information that speech errors can give to the psychologist interested in underlying mechanisms, and a thorough presentation of rhythmic processes in speech. This leads neatly into three chapters outlining the development of language. There is emphasis on communication skills, understanding and semantics.

The last three chapters of the book are perhaps the most interesting. Building on earlier discussion, they cover reading, language and the brain, and language and thought. The chapter on reading provides a nice summary of experimental work integrated with an interesting model. Language and brain links together a number of rather disparate areas including aphasia, localisation and lateralisation, and species-specificity in a comprehensive manner. The final chapter seems a little unhappy—considering linguistic diversity with an emphasis on Black English and linguistic determinism: language and thought.

Each chapter is well summarised and contains suggestions for additional reading. Good indices and a bibliography complete the book. With the exception of coverage of 'sociolinguistics' (which seems to be social psychologists using language variables as indices of social processes rather than psycholinguistics proper), the book is an excellent text for specialised psycholinguistics courses. Underplaying of relevant AI work and omission of much European work—notably the work on thinking and language by investigators such as Wason and Johnson-Laird—suggest that the integration that the umbrella of 'cognitive science' offers should not be ignored.

A. Conway

A. Conway is a Lecturer in the Department of Psychology at the University of Bristol, UK.

Animal behaviour

HERE are reviewed two new textbooks in animal behaviour aimed at the American undergraduate market. Both assume little or no previous knowledge of anything, and both are written in a chatty, mildly condescending manner, eschewing complexity and encouraging a belief in simple solutions. Both also, as befits the type, are full of absurd and redundant illustrations, although the prize here is unquestionably taken by Seay and Gottfried—with a picture of a supermarket; of numerous smiling infants, some busily taking in nourishment; of adolescents standing on street corners or, if of different sexes, sitting

in one another's laps; of a teacher looking at a blackboard, visitors to a science museum looking at an exhibit, and Jean Piaget looking at children playing. The differences between the two books, however, are greater than the similarities.

D. A. Dewsbury's *Comparative Animal Behavior* (McGraw-Hill: New York and London; £14.95) is the more conventional of the two, essaying a relatively straightforward account of psychological, ethological and (to show we move with the times) sociobiological approaches to the study of animal behaviour. The book has many virtues. It covers a great deal of ground (some of it quite out of the way), seems to be reasonably accurate, and does not always settle for received opinion. Although extremely diffident about it, for example, Dewsbury is willing to

Soon to be available as a Student Paperback:

Plant Virology: The Principles

Adrian Gibbs and Bryan Harrison

This modern exposition of plant virology aims to describe the framework of knowledge about plant viruses and to outline the methods used in obtaining this knowledge. The numerous illustrations include superb electron micrographs—many of them original—and other photographs as well as numerous line diagrams. Thus the framework of the subject is provided and much practical information on how plant viruses can be studied is given.

'The production is excellent, with clear type and illustrations. . . .

Unquestionably this book is a valuable addition to the few on plant viruses and will be welcomed especially by the small band who teach the subject at English-speaking universities.' — *Nature* (of the cloth edition)

Paper £9.75

Publication May

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An introduction to these physiological processes that measure time by some means and do so in relation to environmental time-cues, such as high tide or sunset. This is the first work of its kind intended for the biologically informed but non-specialist reader.

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P. J. Reay

This book brings together some of the more recent advances in aquaculture in order to explore the biological basis of this increasingly important industry and the role of the biologist in its future development.

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Publication March 23rd.



Edward Arnold
41 Bedford Square,
London WC1B 3DQ

challenge the conventional assessment of the nature-nurture issue as a pseudo-question. But, for my taste at least, these virtues were outweighed by one pervasive vice: Dewsbury talks down to his readers. The book is permeated with an attitude of 'not in front of the students'. Whenever a topic shows signs of becoming interesting, Dewsbury backs off as if such complex matters were far beyond our capacity to understand. Thus conclusions are proclaimed, but we are never told why they are reasonable. Most biologists, no doubt, reject the theories of Lamarck and of Wynne-Edwards, but it should surely be possible to explain why they do so. Theories must be taken on trust, as they are rarely explicated, certainly not if they contain a hint of mathematics. The concept of an Evolutionary Stable Strategy, however, as Richard Dawkins has shown, can be explained, with numerical examples, in a manner intelligible to a wide range of readers. Dewsbury makes no attempt to do so, and the student must therefore take his word for it that 'the approach appears to have great potential'. Sometimes Dewsbury's reluctance to enter into complicated detail will surely cause much worse confusion than that which he was trying to avoid. Unless already familiar with the problems of navigation, for example, the reader will be quite unable to understand from his account the distinction between Kramer's map and compass theory and Matthew's sun-arc hypothesis of bird navigation; and the sense in which the former is addressed to a simpler question than the latter. Once again, however, it is a reasonably simple matter to illustrate the distinction by reference to the experimental evidence which suggests that Matthews is probably wrong.

Authors are apt to feel hard done by when reviewers complain about the omissions in their books. Dewsbury would perhaps insist that he had no room to go into the necessary detail, but this is largely because he has used his space unwisely. He seems to be obsessed by the need to cover the field, without stopping to ask what should be included in the comparative study of animal behaviour. It is surely reasonable to restrict it to genuinely comparative work, or that which emphasises adaptive function and other evolutionary concepts. It does not need to include the standard facts of sensory and perceptual psychology, conditioning and learning, and physiological psychology. These, as Dewsbury reminds us, are taught in other courses. But this does not deter him from devoting half-a-dozen of his 17 chapters to a necessarily rather cursory survey of these topics. He recounts

some intrinsically interesting facts, but there is little attempt to set them in any theoretical context. His book may be quite useful, but only if students are required to read other matter which will make them think harder.

B. Seay and N. Gottfried, in *The Development of Behavior: A Synthesis of Developmental and Comparative Psychology* (Houghton Mifflin: Boston and London; \$15.50), do not at least waste space or time in the provision of potted information on such standard topics. On reaching the end of their book, however, I found myself at a loss to recall what they had told me. Isolated examples came to mind: an analysis of shopping in a supermarket (hence the picture) in terms of 'super-schemas'; an assurance that love is not a mere epiphenomenon of sex, but a basic need in its own right; and, in case I did not know it, the information that 'lovers also engage in precopulatory behaviour, with participation in sexual intercourse dependent upon local custom and the stage of development of their relationship'. These stood out from an otherwise rather humdrum assortment of standard topics, some superficially recounted facts, and many more uncritically accepted theories. Bowlby's views on attachment, Whorf's on linguistic relativity, and Seligman's on preparedness in learning are all presented to us without any serious suggestion that they might not stand

up to critical examination. The book starts with a series of chapters on 'Phylogenetic Set', 'Ontogenetic Set' and several more sets, defined as 'pre-disposing influences on behavior'; while Part II, just to be different, is organised around various behavioural systems, such as affection and social organisation, which, the reader is asked to remember, are all affected by an interaction of all these sets.

As its subtitle suggests, the book is a curious mixture of comparative psychology and ethology on the one hand and human developmental and social psychology on the other, with a dash of social anthropology thrown in for good measure. But it is not, alas, a synthesis. Throughout the book, we are switched back and forth from studies of animals to the study of man, with little discussion of the relevance of one to the other. It is not so much that the authors are rash or glib in their willingness to generalise, as that they do not involve the student in any serious consideration of the issues involved. Unlike Dewsbury, they have not chosen so to fill their book with facts that there is no room left for discussion: the space is there, but it is largely filled with platitudes.

N. J. Mackintosh

N. J. Mackintosh is Professor of Experimental Psychology at the University of Sussex, Brighton, UK.

Sensory physiology

Two recent books on sensory physiology have a similar aim and are therefore reviewed here together. The publishers have to be congratulated for producing two compact, well printed and well illustrated books.

Jacqueline Ludel's *Introduction to Sensory Processes* (Freeman: San Francisco and London; hardback \$17, paperback \$9) has the advantage that it is written by one author and this gives uniformity of style. She goes to great efforts to define accurately every new term, often illustrating the point with a clearly drawn figure. The book has a considerable number of colour illustrations which do help understanding. She has mastered the art of a good teacher—tell the student everything three times without boring him. This is a book to be read slowly and thoughtfully. It is reasonably up to date and certainly adequate for an introduction to the subject of sensory physiology.

Fundamentals of Sensory Physiology (Springer: New York and Berlin; paper-

back OM 33.60, \$16.80) is very well edited by Robert Schmidt so that, although there are seven different contributors, the style is uniform. As each section is written by an expert in the field it is accurate and fairly up to date. Again, each new scientific word or definition is carefully defined and printed in bold type. At the end of each chapter there are a number of thought provoking questions which ensures that the reader has thoroughly understood the contents.

Both books give excellent references, readily obtainable, for further advanced reading. Teachers of the subject will wish to have both books to hand as there are different teaching points and illustrations in each of them. Individual readers will have a hard task deciding which one to purchase as both are so good. Libraries will wish to have a copy of both. Both will appeal to physiologists, psychologists and scientists in industry dealing with man-machine interactions—for example, photography and sound recording.

F. W. Campbell

F. W. Campbell is Reader in Neurosensory Physiology at the University of Cambridge, UK.